CLAUDE BERNARD AT THE AGE OF FORTY-FOUR
(1857)

Plate I

Claude Bernard

Physiologist

By

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[1] For example, a reference to page 24 of volume ii of the *Phenomena of Life Common to Animals and Plants* would be given as xvii, 24. This system of reference is adopted in Dr. Roger de la Coudraie's table of the subject matter of Bernard's published lectures, essays, etc. (xix, 96). For a more complete bibliography of Bernard's scientific works, including pamphlets, articles in journals, etc., see the bibliography of Godefroy Malloizel (xix, 335–80), or the references listed at the end of the present volume.

PREFACE

WHAT WAS undertaken as an academic biography of a subject in whose contributions to science I was professionally interested has, by the mere accumulation of detail, grown into something with more texture and pattern than I had anticipated. The hero's story begins romantically with his humble but vigorous origins and with the youthful exuberance and intrepidity with which he set off to the great city, having for his principal luggage the manuscript of a play which he had written during his apprenticeship to a pharmacist. Then follow the sturdy recovery from what must have been an enormous disappointment when he was told, perhaps too harshly, that his literary effort showed no promise; the long struggle of the early years against narrow means; and the final attainment of a medical degree unaccompanied by any desire to practise medicine.

Then comes the core of solid, unchallengeable achievement of the middle years while he followed his bent as an investigator in the science of physiology. But success is marred by a streak of pure ill-luck, the unfortunate marriage, which, with the death of his infant sons, must have come close to tragedy for one who, as far as we may judge from the devotion of his scientific associates, had something approaching a genius for human relationships. At the height of his powers he is interrupted by an illness which for a time banishes him from the laboratory. The latter part of his life is crowded but the great days are over. There are the social and political adventures, the great scientific prestige, the friendship upon which so

much ink and paper were spent without its essential superficiality ever being overcome, the colds and sciatica; and, finally, the last wretched illness, the faithful ministrations of his devoted pupils and the great public funeral at the expense of the State.

The detailed narrative of his discoveries brings to light not sureness and precision, but a sort of intuitive power in investigation. There are many blunders in spite of much-vaunted caution; but even his mistakes pointed the way to the truth, and the structure of physiology rests on sure foundations which he laid. In his exposition of the experimental method he really succeeds in showing how the mind of a scientist works, and he leaves a document which is fascinating to every experimenter working with biological material. His philosophical position is not intrinsically important, but it is interesting as being typical of his generation. Within the limits of his own science he achieved one magnificent generalization which at the present moment exerts a powerful influence upon physiological investigation.

There are many essays on the life and work of Claude Bernard, but only one book-length biography in English, that of Sir Michael Foster, a first-rate nineteenth-century account of the main events of his life and of his discoveries, but a little meagre in detail and now out of print. The popular French biographical essay of Jean Louis Faure has for its principal theme a comparison between Bernard and Pasteur in which Bernard comes out a poor second and Pasteur grows into a legendary figure. Claude Bernard in his lifetime towered above Pasteur, and, on a sufficiently broad view, there is no reason to reverse their respective positions to-day. The official academic biography written by M. van Tieghem for the

Academy of Sciences is more authoritative, but very brief. All subsequent biographers draw on Renan's reception speech at the French Academy to the point of echoing his slight inaccuracies. Although Renan was an intimate friend of Bernard, he seems to have relied finally as much upon information from other friends as upon his own recollections. For example, the description of Bernard's country estate, which he quotes and which every one quotes after him, is an excerpt from a letter to Madame Raffalovich, the full text of which I have given. It is strange that the Raffalovich correspondence has been neglected by biographers because it throws so much light on the last ten years of Bernard's life.

The original letters are in the library in the Institut de France, and I am very grateful to the librarian, M. Bouteron, for having permitted me to examine them. I wish also to thank M. Henri Langier of the Sorbonne for information regarding Bernard's family. Finally, Dr. Genty, librarian of the Academy of Medicine in Paris, has been indefatigable in digging up new facts about Bernard's life, and he has been kind enough to allow me to draw upon his stores of information as I saw fit. Professor d'Arsonval, Bernard's pupil and his successor at the Collège de France, has graciously sent me reprints from which I have gleaned several new facts. My chief source, however, has been Bernard's own published works, and I have appended a key to my references to their eighteen volumes. I am obliged to my wife for certain suggestions with regard to Part III. I am grateful to Dr. Sigerist who has allowed me to use parts of my article on "The Contemplative Works of Claude Bernard," published in the *Bulletin of the Institute of the History of Medicine*, and to Mr. Paul B. Hoeber,

publisher of the *Annals of Medical History*, who has granted me a similar permission regarding my article on "Claude Bernard as a Dramatist."

Above all I am indebted to Monsieur J. Devay, Bernard's great-nephew, who most hospitably received me in the very house where Bernard was born and which even to its furnishings remains much as it was a hundred years ago. He gave me to taste of the wine grown from the descendants of the vines whose vintage gave Bernard so much pleasure (and Pasteur so much pain), and allowed me to rummage through the relics of his great-uncle on an extremely hot day in June, 1935.

J. M. D. Olmsted

Berkeley, California.

FOREWORD

The spirit of Claude Bernard still illuminates medicine. His method and his discoveries are extolled by scientists all over the world. Nevertheless, Bernard, as a man, remains almost entirely unknown. While Pasteur, immediately after his death, became a legendary figure, Bernard was forgotten at once by the public, in spite of the glamour of the great national funeral that France for the first time accorded a man of science. The events of his life vanished in the mist of the past, like those of the lives of his ancestors, the humble peasants of the hills of Beaujolais.

To-day, however, after more than a century, a voice arises from the shore of the Pacific Ocean and tells us the marvellous story of the apprentice in a small pharmacy of a French city, who, after having failed as a playwright, became one of the greatest scientists of the world. Dr. Olmsted wrote this book at the University of California. As he himself is a physiologist, he describes the discoveries of Bernard, his method, his attitude towards the mystery of life, with a profound knowledge of their importance. But also with a love inspired by their beauty. At the same time, he discloses to us the man hidden under the academic gown. Dr. Olmsted went from Berkeley to the French village where still stands the poor little house of the great man. And he describes the humility and the triumphs of Bernard's existence, his friendships, his sorrows, his misery. In this manner, he has resurrected a man whose wonderful intelligence has helped each one of us. For Bernard is the

father of modern medicine. Before him, medicine was purely empirical. He is responsible for the introduction of the scientific method in the art of healing.

To the narrative of the life of such a man, no one can remain indifferent. For the progress of mankind depends on the advent of geniuses. And we may hope to discover in their lives the secret of their making.

Is it not strange that nearly at the same time, and in the same region of France, three great men of science were born? Ampère in Polémieux, Bernard in Saint-Julien, Pasteur in Dôle. Each one came from obscure stock of French peasants. These peasants were frugal, religious, strong, hardworking men and women.

The formal education of Bernard was incomplete. He had to work for a living during the years which young men usually spend in schools or universities. But gifted ancestors and higher education are not indispensable to the development of genius. Besides intelligence, genius consists of many qualities of the mind that are not intellectual. Bernard was endowed not only with a great intellect, but also with the intuitive power of an artist. He had a profound æsthetic sense. The beauty and purity of his style, when he described his discoveries, were doubtless the expression of this inner feeling. The story of his life teaches us several valuable lessons. Greatness may spring from nowhere. Unploughed fields sometimes yield a rich harvest. Both logic and intuition are essential qualities of the discoverer.

It is indeed fortunate that Dr. Olmsted has

succeeded so well in calling up from the past the great figure of Bernard. I am happy to introduce this book to the American public, and also to all those who, in other countries, are interested in the highest achievements of the human mind.

ALEXIS CARREL

Part I

LIFE

"*Quand nous considérons la vie de l'homme relativement à la durée du milieu cosmique qu'il habite, elle doit nous paraître un instant dans l'infini du temps.*" (xviii, 171.)

CHAPTER ONE
YOUTH

Je me souviens avoir connu, dans la campagne que j'habitais étant enfant, des paysans qui avaient le secret de composer des pommades soi-disant merveilleuses contre la gale.[1]

"IN THE YEAR 1813, on the 12th of July, at seven o'clock in the morning, Pierre Jean François Bernard, property owner in the village of Saint-Julien, canton de Villefranche (Rhône), France, appeared before the mayor of the commune and made declaration that between three and four o'clock of the morning of that day there was born to him by his wife, Jeanne Saulnier, a male infant to whom he wished to give the Christian name of Claude." This is the civil record of Claude Bernard's birth to be found in the mayor's office at Saint-Julien. He was a first and only son, although not an only child, for a daughter was born later. Six days after his birth he was baptized by the parish priest in the presence of relatives and friends, his godparents being his grandfather, Pierre Claude Bernard, and an aunt, Marguerite Baloffet.[2]

The grandfather after whom the child was named

[1] xi, 576.

[2] The certificate of baptism reads: "In the year 1813 on the 18th of July, I, the undersigned, baptized Claude, born on the 12th of the same month, the legitimate son of Pierre Jean François Bernard, property owner in the commune of Saint-Julien, and his wife, *née* Saulnier; the godfather was Pierre Claude Bernard, grandfather, property owner at Arnas, and the godmother Marguerite Baloffet, of the Bernard family, an aunt, and also in presence of Pierre Paschal Burdin, property owner at Oully, of Pierre Louis Marion, property owner at Glèze, who have all signed with us this day and year, with the exception of Marguerite Baloffet, who when questioned declared herself unable to do so. Clément, *curé*."

had lived in Regnié-en-Beaujolais in the middle of the eighteenth century and had married Jeanne Baloffet. Four sons were born of the marriage, the youngest of whom was Pierre Jean François. The family moved later to Arnas where Pierre Jean François married Jeanne Saulnier on November 10, 1807. Because the bride's mother owned property at Saint-Julien-en-Beaujolais, Pierre and Jeanne went there to live in her mother's house, and their son was born in an upstairs room of the old brown-tiled farmhouse on the brow of the hill, Châtenay, overlooking the village of Saint-Julien with its cluster of tawny yellow houses, its church and poplar-bordered brook. His infant games were played in the courtyard under the square sharp-roofed tower of the pigeon loft. Except for an orchard just beyond the courtyard wall, vineyards covered the hills and filled the valleys as far as the eye could see. The old house is still, after a century, in the possession of a member of the family, Claude Bernard's great-nephew, M. J. Devay. The upstairs room, even now, is furnished with the Empire bed in which Claude Bernard was born, but the bronze eagles originally surmounting its four posts are missing. They were removed in 1815 at the downfall of Napoleon. In 1935 a plaque marking the house as the birthplace of a great scientist was placed over one of the courtyard windows.

The simple statement that Claude Bernard's father was a winegrower does not give an adequate account of the origins from which the great physiologist sprang. Pierre Jean François Bernard was no illiterate peasant spending his existence grubbing among his vines. Having come into possession through his wife of two vineyards on Chatenay he entered into partnership in the wine business about the end of the First Empire

when all commercial ventures were extremely hazardous. The result was disaster, the vineyards were sold; the elder Bernard contracted debts which he bequeathed to his son, and, to augment his income, he became the village school-master. During the slack seasons the children of the neighbourhood were entrusted to him for instruction in the elements of the French language, history, geography and arithmetic. There were no state schools at the time and instruction was given in Bernard's own house.

There are no references in Claude Bernard's published works to his childhood at Saint-Julien except the one quoted at the head of this chapter. There is a unanimous tradition[1] that he was deeply attached to his mother. In later years, when she was a widow, although she refused to come to live in Paris, preferring to remain in the country with her daughter and her grandchildren, she nevertheless derived great satisfaction from her son's position in the capital.

[1] All his biographers dwell with emphasis on Claude Bernard's tender relations with his mother whom they insist upon describing as a widow. They seem to have taken their cue from Renan who said in his speech before the French Academy when he succeeded to Bernard's chair: "Bernard lost his father early; during his tender years, as at the beginning of the lives of nearly all great men, we find the love of a mother whom he adored and by whom he was adored" (xix, 17). M. van Tieghem uses almost the same phrases and adds: "It was under the vigilant eye of a devoted and affectionate mother that his childhood was spent" (*Mém. Acad. d. Sc. Inst. de France*, 52: v, 1914). M. Barral goes further and intimates the effects which this might have had on Bernard's character: "He was not a young man when he married; it is certain that sex did not play an important part in his youth. To placidity of temperament one must add his education and surroundings and his affectionate relations with his widowed mother" (Preface to Bernard's *Arthur de Bretagne*). Sir Michael Foster is more restrained but places the death of the elder Bernard as occurring during his son's attendance at medical school (*Claude Bernard*, pp. 5, 10). As a matter of fact, the civil records at Saint-Julien give us not only the day and year but even the hour of the decease of Pierre Jean François Bernard; it occurred at three in the morning, May 9, 1847, when he was sixty-two. Claude Bernard at this time was thirty-four, had been married two years and had an infant son of his own. Renan and Barral were very close personal friends of Bernard, and weight must be given to their testimony as to his especially affectionate relations with his mother, supported as it is by that of others. Because she had survived her husband by twenty years it was natural for the friends *who knew Bernard in later life* to think of her as a widow. They were right about his feeling for his mother, but wrong in their explanation of it.

According to the testimony of his academic associates, he went down to see her as often as he could, even at times when it was difficult for him to take a few days' vacation. She died at the age of seventy-eight, only eleven years before her son, having lived to see his fame fully established. Bernard wrote to Charles Robin in 1871: "I share with you the unhappiness which comes to you in the death of your mother. Three years ago I had the grief of losing mine, and I know by experience that it is one of the greatest sorrows in life."

His feeling for his father seems to have been less marked. It has been suggested that the elder Bernard was away from home a great deal, having to make frequent journeys to Paris in the interests of his wine business, so that Claude as a child actually saw little of him. An associate of Bernard in middle life left it on record that he had never heard him speak of his father. At all events, Pierre Jean François Bernard did not witness more than the very beginning of the recognition which was to come to his son, for he died in the very month and year when Claude Bernard gave his first lecture at the Collège de France as *suppléant* for the celebrated Magendie.

Claude's schooling began when he was eight years old with instruction in Latin at the house of the curé Bourgaud. About 1825 M. Bédouin, who was administering the parish at Saint-Julien, noticed Claude's serious character, had him assist at mass and carried on his instruction in Latin. Two other ecclesiastics who lived nearby at Chatenay, the abbés Garets and Desarbes, also became interested in the boy, and recommended to the priests at the Jesuit College of Villefranche that he be received there as a pupil. At that time the college occupied a part of the ancient

convent of the Visitation, a building which still stands and which was used as a hospital during the war of 1914–18.

The kindest tradition describes the boy at this period as dreamy and serious, always silent and pensive. According to another, he was a very ordinary student, cold, taciturn, mingling little with his fellows, who tried in vain to make him join in their games and were later much surprised at his rise to fame. At Villefranche he was taught Latin, a little Greek, French, arithmetic and geometry; no science, no history or geography, no modern languages except his own. A comment of one of his teachers has been preserved to the effect that he was only an average student; he never liked to read, since it seemed to him a waste of valuable time. Two of his school books are among the relics at Saint-Julien: a French-Latin dictionary, with the word *meretrix* underlined; and an unworn Cicero, bearing in two places the inscription in Greek characters, "Bernard, Claude, of Saint-Julien, Rhône," and the date, 1829. There is in addition, as if in refutation of the criticism of his teacher, a very well-worn and dog-eared heroic romance in two volumes, *Argénis*, by Barclai, published in Paris in 1728, with Bernard's name in pencil on the fly-leaf. He eventually left Villefranche, and went to the college of Thoissey (Ain) for a year.

The boy was now eighteen and his family found that they could no longer afford to keep him at school. In January, 1832, he obtained employment in the shop of a pharmacist, M. Millet, in the faubourg de Vaise of Lyons. At first his duties were simple, sweeping the pavement, rinsing bottles and serving purchasers under the eye of his employer. His youthful pride suffered under the more menial of his tasks, and

in rolling pills and compounding medicines and spent too much of his time dreaming. Claude himself wrote to his father that he could not make up his mind, when he was only nineteen, to spend the rest of his life folding up little squares of paper.[1] He left the pharmacist's shop on the 30th of July, 1833, having served a year and a half. He later obtained a rather lukewarm testimonial[2] from his employer stating that the apprentice had conducted himself honourably and faithfully but omitting to suggest that he had any special talent for pharmacy. A year of enforced idleness followed during which Bernard finished the play, which was published many years afterwards in 1887 under the title *Arthur de Bretagne*.

During his stay at Lyons, if Bernard attended the performances at the other principal theatre, the Grand Théâtre, he had the opportunity of being introduced to the "romantic movement" as it affected the French drama. In 1832 M. Firmin brought his company from Paris and put on *Henri III et sa cour* by Dumas, *Hernani* by Victor Hugo and two plays derived from Shakespeare, *Antony* and *La Jeunesse de Henry V*, he himself playing the principal rôles which he had created in Paris. *Henri III et sa cour* three years before had been the first successful romantic drama to be produced in the capital.

[1] Père Didon, *Rev. de France*, 28: 2, 1878.

[2] The testimonial, still preserved at Saint-Julien, reads as follows: "I, Louis Joseph Marie Millet, the undersigned, pharmacist of the faubourg de Vaise of Lyons, certify that M. Claude Bernard, a native of Saint-Julien, district of Villefranche, department of the Rhône, aged twenty-one years, entered my employ in the capacity of apprentice the first of January, eighteen hundred and thirty-two, and that he left it the thirtieth of July, eighteen hundred and thirty-three, that during these eighteen months he served with honour and fidelity, and therefore I give him this certificate to serve him and to be of whatever use to him it may.

Vaise, the nineteenth of April,
Eighteen hundred and thirty-four.

MILLET."

Hernani in 1830 had been the occasion of the celebrated "battle" in which the classicists and romanticists had actually come to blows in the theatre. The plays derived from Shakespeare followed the fashion set by de Vigny's *Le More de Venise* which in 1829 introduced transcription of Shakespeare to the French stage.

Bernard's play, *Arthur de Bretagne*, might easily be interpreted as the direct result of attendance at performances at the Grand Théâtre during 1832 and 1833. It is a typical romantic drama for it has a well-documented historical plot, it bears marks of having been in part derived from Shakespeare's *King John*, and its style, described by most French critics as "rather emphatic," mirrors the melodramatic extravagance of *Hernani*.[1] It is curious in view of what seems to be the obvious influence of the contemporary drama on Bernard's play to find that towards the end of his life he complained, "Nevertheless my tragedy was classic. It was composed of five acts in which I observed the rules laid down by Aristotle for unity of action, of time and of place, and I fell into the midst of romanticism which did not respect any sort of unity."

The play deals with the struggle of young Arthur in 1202 against his wicked uncle, King John, who had not only usurped from him the throne of England, but was planning to deprive him of his French provinces as well. The scene opens in Brittany at the manor-house of des Roches, for whose young daughter, Marie, the boy Arthur has conceived a deep attachment. Des Roches urges Arthur to fight for his rights against John, and the lords of the provinces also urge him to defend them. He promises, but the fighting

[1] See Olmsted, J. M. D., "Claude Bernard as a dramatist," *Ann. Med. Hist.*, N.S. 7: 253–60, 1935.

goes against him. King John proposes peace, but a dishonourable peace, since he demands that Arthur renounce his claims both to the English throne and to the French provinces. Arthur refuses; he has pledged his word and will not go back on his oath. He is captured and imprisoned. At John's orders the governor of the prison prepares to burn Arthur's eyes out with red-hot irons, but desists at the victim's pleadings. Des Roches plans for Arthur's escape; the bars of the tower window have been sawn through; Arthur is to lower himself by a rope when he hears the hymn to the Virgin. The song is sung, Arthur disappears through the window and Marie (who, as a result of Bernard's ingenious dramatic construction, has penetrated to her lover's prison) is left behind in the dungeon tower. Suddenly, her father, covered with blood, together with the Dauphin of France and a crowd of soldiers, bursts into the room. Treachery! John has learned of the plot through his spies. Des Roches gasps out that he saw John take his nephew away in a boat, saw him plunge his dagger into the boy's heart, heard Arthur's last cry. Faithful old des Roches falls dead at his daughter's feet, and Marie with a cry, "*Oh la mort! à moi aussi la mort!*" falls in an anticlimactic faint as the curtain is rung down on the fifth act.

When he had finished his play, Bernard longed for recognition such as could be gained only in Paris. A lady in Villefranche, where he had attended the Jesuit college, gave him a letter to M. Vatout, at that time friend and librarian to the bourgeois king, Louis Philippe, and in November, 1834, he took the diligence for the capital. M. Vatout received the young provincial with kindness and procured for him an introduction to M. Saint-Marc Girardin, professor

of literature at the Sorbonne, then at the height of his fame as a critic. M. Girardin read the manuscript, then said firmly, "You have done some pharmacy, study medicine. You have not the temperament of a dramatist."

For the time being this was the end of *Arthur de Bretagne*, for Bernard took his critic's advice and abandoned literature as a career. He had not the heart, however, to destroy the manuscript, and although he did not allow it to be published in his lifetime its existence became a legend and Bernard himself referred to it as "this tragedy with which I am reproached." A year and a half before his death he presented the yellowed sheets to the son of an old friend, M. Georges Barral, with the express stipulation that if M. Barral wished to publish it he might, but not until at least five years after the author's death, and that it must bear the statement that it had been read and refused after numerous corrections by M. Saint-Marc Girardin in 1834. M. Barral dutifully carried out these instructions with the result that at the time of Bernard's death there was much mystery about the matter and *Le Figaro* published the following garbled account: "Bernard's tragedy was about Charles VI and in verse. . . . One of his friends who died mad had the courage to learn the famous tragedy by heart and carried it with him to his grave. There is no other copy." The affair was finally cleared up in 1887 when Bernard had been dead for nine years. M. Barral at last removed the manuscript from the iron coffer in which he had preserved it like a precious relic and it was published. The original manuscript is now safe in the Bibliothèque Nationale in Paris.

In his preface M. Barral justifies his publication of

the play on the ground that in it are revealed "the future qualities of the great investigator," but he leaves the reader to ferret out such revelations for himself. It is true that certain aspects of the play seem to be rather personal to the youthful Bernard of the time when it was written. The character of Arthur, in which the chief traits are sense of duty and freedom from personal ambition, is an expression of boyish idealism. The frequent references to religious devotion suggest that Bernard was still acquiescent in the instruction which he had received from parish priests and Jesuit fathers. The love story with its occasional *naïvetés* gives the impression of a lack of sophistication in its author but it also bears witness to his delicacy and sensibility. Arthur's most passionate speech is an aside: "Angel of purity! Oh! let us not inject into her soul the fire which burns and consumes my own." A certain softness, not to say self-indulgence, of temper is suggested in the "purple passage" where King John outlines the pleasures of his court, the chief of which would appear to be the licence to lie abed in the morning (a luxury impracticable for an apothecary's apprentice). "The sun is already high in the sky when softly extended on your couch you are still cradled in sweetest dreams." But we must not forget the admirable industry which went into the accomplishment of Bernard's self-set task. What he lacked in inspiration had to be made up in honest toil. We know from M. Barral that he had at first intended to write a tragedy in verse; but he found his spirit "stubborn in the composition of verses." Nevertheless, he embellished his prose drama with one short song which, although it is no more than a recollection of the hymn to the Virgin coming from his days as "*enfant de chœur*," perhaps serves as well

as anything to evoke the qualities of the young man who made this valiant but unsuccessful attempt to take literature by storm:

Salut, astre des mers,
De Dieu mère féconde,
Humble vierge en ce monde
Qui tiens les cieux ouverts!

C'est le divin salut
Que t'addresse un bel ange;
En Marie Ève change:
C'est la paix, le salut.

Rends le jour à nos yeux,
Délivre des coupables;
Donne à des misérables
Tous les trésors des cieux.

Montre-toi bien la mère
Du Dieu compatissant
Qui, pour nous, dans ton sang
Puisa la vie amère.

Modèle de douceur,
Aux vertus singulières
Viens mouiller nos paupières,
Épurer notre cœur.

Assure dans sa voie
Le pauvre voyageur;
Qu'il ait dans le Seigneur
Son éternelle joie!

Au Seigneur, tout-puissant,
Un fils chéri du Père,
À l'Esprit de lumière,
Un seul cœur, un seul chant!

CHAPTER TWO

FOUNDATION OF A SCIENTIFIC CAREER

C'est chez la jeunesse, en effet, que se trouve une force vive qu'il faut utiliser tout de suite, au lieu de la laisser s'égarer dans des directions sans issue, ou se perdre dans des luttes stériles. Il faut donc donner aux jeunes gens tous les moyens d'études.[1]

BERNARD ACCEPTED his defeat in the field of literature and even followed at once the advice that was offered with the rejected manuscript. In the autumn of 1834, when he was twenty-one, he entered the Medical School in Paris. The tradition handed down from his contemporaries describes him at this time as taciturn, awkward in manner and inattentive at lectures. He was definitely not a showy student. Like his early schoolfellows, the medical students who attended the same courses seem not to have expected him to go on to a brilliant career, and his instructors were inclined to regard him as merely lazy. The lack of any real appreciation of his latent gifts may have fostered in him a tendency to be critical of the formal instruction provided. At any rate, his reserves of energy found their outlet in the study of anatomy and more particularly in dissection where his clever hands soon made him feel his power.

His thorough grounding in anatomy and his skill in dissection are in fact responsible for much of his later success in experimenting on living animals. He realized that "anatomy is the basis necessary for all

[1] xi, 475.

medical investigation";[1] but he insisted that "anatomy in itself teaches nothing without observation of the living."[2] His interest in dissection appeared in innumerable ways throughout his entire scientific life. It was expressed early, for example, in his contributions to anatomical textbooks, and many of his early short papers were purely anatomical.[3] His lectures give the most exact details of the dissections employed in his experiments. So essential did Bernard consider this aspect of experimental physiology that as late as 1873 he began work on a treatise on experimental technique.[4] When this book was finally published, only the first twenty chapters had been reviewed and corrected by Bernard himself, and this part contains numerous illustrations of instruments, scalpels, scissors, etc., as well as drawings of dissections.

Physiology as a separate science at the time that Bernard was a student was practically non-existent. He stated[5] that before his time physiology was always confused with anatomy since these two subjects were taught together during the first half of the nineteenth century. It was only after 1850 that physiology had progressed sufficiently to demand separate teaching, separate methods of investigation and separate laboratories. The German physiologists who dominated this science about the middle of the century, Ludwig (1816–95), Helmholtz (1821–94) and du Bois Reymond (1818–96), had not yet begun their work in 1834. The foremost physiologist in Germany at this time was Johannes Müller (1801–58), and while

[1] viii, 182. [2] viii, 200.

[3] E.g., on the dispositions of the muscular fibres in the inferior vena cava of the horse (1849); anatomy of a two-headed calf (1849); false hermaphroditism observed in a kid (1850), etc.; *v. Mém. Soc. de Biol.*, 1 (*C.R.*): 33, 126, 145, 1849; 2 (*C.R.*): 128–30, 1850. In 1854 he mentioned having devised a modification of Hunter's forceps in 1847 (xix, 342).

[4] xv. [5] xiv, 52; xi, 539.

Bernard was attending medical school Müller was just writing his famous *Handbook of Physiology* (1834–40) in which "the results of comparative anatomy, chemistry and physics were for the first time systematically brought to bear on physiological problems."[1]

In France Magendie (1783–1855) stood practically alone as the representative of experimental physiology during the first half of the nineteenth century, but he, unlike Müller and the other German physiologists, founded no school, nor did he have a following of students who had come even from foreign countries to work under a famous teacher. Magendie was a busy physician in charge of a *service* at a hospital, the Hôtel-Dieu, as well as Professor of Medicine at the Collège de France where his experimental work was done. Bernard later gave what he thought were the reasons for the state of physiology in France during his student days: "The material obstacles which experimental physiology encountered in France are the necessary consequences of the feeble scientific importance given it. It is quite natural to neglect a science which one does not know or which one combats, and to assign it only the smallest place in the curriculum."[2] He placed the blame for this state of affairs chiefly on Cuvier (1769–1832) whose influence was very great. He said that Cuvier not only denied the independence of physiology, but because of his vitalistic bias was ignorant of its methods and made the accusation that vivisection led to error.[3]

In spite of his apparent indifference to lectures and his rather specialized interest in dissection, Bernard passed his first landmark as a medical student by becoming *externe* in 1836. It was a hard struggle, for he had almost no financial aid and he was obliged to

[1] Singer, C., *The Story of Living Things*, p. 388. [2] ix, 143. [3] ix, 234.

resort to tutoring to eke out his slender resources. He taught natural history in a girls' school where his fellow student, Lasèque, taught literature and where his future lifelong friend, Davaine, was also a teacher. In the competitive examination for *interne* in December, 1839 (a *concours* which only the best students were able to pass) Bernard was a successful candidate, but ranked twenty-sixth out of twenty-nine. One's rank in examination is by no means a sure index of success in life. Pasteur, thirteen years later, presented himself for examination before the faculty of Dijon for the baccalaureate in mathematical sciences. He was placed fifteenth out of twenty-two, his chemistry being considered "mediocre." Having become *interne*, Bernard was now embarked on the last four years of his medical studies, which would culminate in the presentation of a thesis for his degree.

Dr. Genty states that Bernard followed the *services* of Falret, Valpeau and Maisonneuve before he was attached to Magendie at the Hôtel-Dieu. A few references to his activities at this stage of his career may be gleaned from scattered passages in the published lectures. He occasionally draws for illustration on his recollections of what he saw as *interne* in the Paris hospitals. He records attending an operation by M. Manec at the Salpétrière in 1840 involving the resection of the pudic nerves. This being before the era of anæsthetics, the patient was fully conscious and his protests indicated that he misplaced the site of his injury.[1] Bernard also remembered vividly some cases of the removal of tumours.[2] At the Charité in the service of M. Rayer he had the opportunity to observe many cases of diabetes,[3] and at the Hôtel-Dieu under Magendie he had under observation a

[1] iv, 296. [2] ii, 57; xi, 66, 119. [3] ii, 99, 299.

patient who was losing considerable quantities of cerebrospinal fluid each day. Magendie at this time was particularly interested in the composition and function of cerebrospinal fluid and Bernard tells of the procedure which he adopted in collecting the fluid from a cadaver for Magendie's use.[1] He was also brought into contact with awakening controversy as to the efficacy of phlebotomy. Magendie, who was apt to be in advance of his time, opposed bleeding in cases of pneumonia. Bernard says:

> In 1841, when I was *interne* at the Hôtel-Dieu, in the *service* of Magendie, he had already been for a long time opposed to bleeding in pneumonia cases, believing that bleeding, far from having a good influence on the outcome of the disease, on the contrary only made convalescence more difficult. This idea, which was out of harmony with practice in all the other medical services, seemed so daring and even rash that often, without Magendie's knowledge, the *internes* on duty believed themselves in conscience bound to go contrary to the master's prescription and perform the routine bleeding of pneumonia patients who were brought in breathing with difficulty.
>
> At this time, Grisolle presented at a meeting of the Academy of Sciences his "Practical treatment of pneumonia" in which he established by statistics and observations that bleeding is the best treatment for pneumonia. Magendie, who was a judge in this competition, protested: "I don't bleed pneumonia patients at the Hôtel-Dieu; they recover; what's more, their convalescence is quicker." Breschet, his colleague, who was also on the judging committee, could not restrain a smile: "You do not bleed your patients, it is true," said he to Magendie, "but your *internes* bleed them behind your back."
>
> The next morning Magendie complained bitterly to me of this violation of his orders; and I must say that after that no more patients were bled.[2]

[1] iv, 495, 503.

[2] xiv, 225-6.

François Magendie was at this time nearly sixty years of age, and had a reputation for a gruff, abrupt and disconcerting manner. According to Renan, he noticed Bernard's skill in dissection almost as soon as the latter came into his *service* as *interne*, and although he scarcely knew the young man's name, he shouted one day from the end of the table, "See here, you, I'll take you as my *préparateur* at the Collège de France!" There is reason to believe, however, that the choosing of Bernard by Magendie was not quite so precipitate as Renan's account would lead one to suppose. We have Bernard's own statement that he did not become Magendie's *préparateur* until 1841.[1] Furthermore, he mentions that he attended Magendie's course of 1839 at the Collège de France and was a frequent visitor at his laboratory there.[2] Speaking of Magendie's experiments on sensitivity of the anterior roots of the spinal nerves, he says: "I was witness to the facts that he had pointed out in his lectures; I saw the preparations at close range; I even touched them." Perhaps he was the student to whom Magendie referred when he said in his lecture on May 8, 1839:

> At the end of my last lecture, one of the students in this course came to ask me if all sensitivity really was extinguished after section of the posterior roots. The student thought that perhaps the muscles were still sensitive. I answered him by telling him to repeat for himself my experiment on the animal which had just served for my public demonstration of these facts. . . . He accepted the challenge and was only convinced after having made deep incisions into the parts whose sensitivity had been paralysed; and he pushed his incredulity so far as to pinch the anterior root of a spinal nerve whose posterior root had been cut.[3]

[1] xiv, 31. [2] iv, 36.
[3] Magendie, F., *Leçons sur les fonctions et les maladies du système nerveux*, 2: 74.

In fact, it seems clear that Bernard's interest in and association with Magendie went back to a time even earlier than his passing of the examination for *interne* which brought him on Magendie's *service* at the Hôtel-Dieu, and that at least another year passed before he received the appointment as *préparateur* at the Collège de France.[1] Van Tieghem's account of the selection of Bernard by Magendie for this post is in agreement with this view. He says that Magendie had been so difficult to get along with at the Hôtel-Dieu that Bernard had been very disheartened. His discouragement had become so profound that he was tempted to renounce science and, as soon as he had finished his medical course, return to his native town to practise medicine. Happily, another of his instructors, M. Rayer, realized the difficulties developing in the situation and intervened. Magendie, now better informed, became less harsh, and it was then that he made the young *interne* his *préparateur* at the Collège de France.

That there were difficulties in the relationship between the professor and his new assistant is borne out by an account of Magendie's characteristic attitude to his pupils which Bernard gave many years later, when he had himself become a professor, in the introductory lecture of one of his courses. He said:

> There was no resemblance between M. Magendie as a casual acquaintance and M. Magendie in the laboratory; in these two settings he was two quite different people. All who knew M. Magendie remember how affable and kindly he was with the world at large. But in the laboratory and in his scientific relationships his character changed completely and automatically took on the colour of his

[1] Claude Bernard "entered Magendie's laboratory as a voluntary assistant. In the second year of his interneship, he became his official *préparateur*": de Bariches, A., *Les Savants contemporains.*

> peculiarities as a scientist, that is to say, of his deep antipathy for all argumentative discussion. When a young man, full of youthful enthusiasm, came to consult M. Magendie about ideas, about plans for work on which perhaps he based the highest hopes, he always suffered at M. Magendie's hands complete disillusion. This advisory frankness was often taken very ill. M. Magendie, however, believed that it was a useful test which prevented later and more painful disappointments. . . . If someone else approached M. Magendie, not with ideas or plans, but with a fact, with an experiment of which he described the results, the first reaction of M. Magendie was always a contradiction: What you tell me is impossible, he would say; you are mistaken. This was a sort of ordeal to which M. Magendie seemed to subject all those whom he did not know. If, however, you opposed him and, sure of the truth of what you were telling him, tried to make him see it, he did not refuse to listen; on the contrary, he wanted persistence, and if you performed a good experiment proving clearly what you had told him, he was the first to acknowledge it cheerfully, and congratulate you, and thereafter you had his respect and sympathy.[1]

Once the old professor's peculiar disposition no longer formed a barrier between them, however, nothing could have been more fortunate than this association between Magendie and Bernard. Instead of formal instruction, a planned exposition of the known facts of medical science, Magendie attempted at the Collège de France to show his audience experiments which had never been tried before. In his introductory lecture on the nervous system in 1838 he said: "Gentlemen, the medicine I am to teach you is a science in the making." Bernard had come under the right man and had found the environment best suited to the development of his genius. Magendie

[1] iii, 28–9.

gave his *préparateur* a free hand and Bernard's dexterity had scope for its effective display. At the third or fourth demonstration, so the story goes, Magendie left the room saying in his usual surly tones, "Well, you're better than I am."

There has recently come to light a remarkable document in the form of a note-book which Bernard used during his interneship for recording physiological memoranda.[1] This note-book was presented in 1931 to the Academy of Medicine in Paris by M. J. Devay, Bernard's great-nephew. The entries are of two sorts: first, abstracts of Burdach's treatise on physiology and of memoirs by Gall and other contemporary investigators; and secondly, outlines of the known facts of certain physiological processes, e.g., respiration, circulation, nutrition and secretion, along with queries as to lacunæ in the existing knowledge of these subjects. In connection with these latter are indications of experiments performed and suggestions for future work; and these suggestions turn out to be prophecies of inquiries which for many years were to engage Bernard's attention. For example, his query as to the relation of other functions, such as olfaction, to respiration was dealt with in his lectures on the nervous system in 1858; a query as to whether the function of the liver does not include more than the secretion of bile foreshadowed his memorable treatise on that organ; others, on the seat of animal heat, on the resistance of warm-blooded animals to high temperatures, and, in particular, on the influence of nerves on animal heat, were to be the subjects of some of his most famous experiments. This is not the ordinary student's note-book. It bears witness to Bernard's native scientific imagination,

[1] Roger, H., *Presse méd.*, 41: 1785, 1933.

which had not been dulled by three years of routine in the prescribed courses of medical education and which almost certainly had been fully awakened by attendance at Magendie's course at the Collège de France in 1839.

This is borne out by the fact that many of Bernard's early investigations were repetitions of previous experiments by Magendie, especially those involving the cutting of nerves.[1] During 1841 and 1842 he seems to have been engaged in an attempt to clear up the controversy which had arisen between Magendie and Longet in regard to the sensitivity of the anterior roots of the spinal nerves. In his classic work in 1822 Magendie had shown that the posterior roots were sensitive and he intimated that the anterior roots were not sensitive. In his course in 1839, which was attended by both Bernard and Longet, he clearly demonstrated that the anterior roots were very sensitive. This discrepancy, however, did not worry Magendie. His extreme scientific empiricism demanded only that he record the facts as he observed them. His distrust of "theorizing," born of his repugnance for "systems" which he considered to have retarded the advance of science before his time, actually forbade him to attempt to reconcile contradictory results. If he obtained one result in 1822 and another in 1839 it was all the same to him. He trusted to future experiments to provide the missing explanation. Far from being ashamed of his complete lack of method in the conduct of research, he was proud of it. He told Bernard: "Every one compares himself to something more or less majestic in his own sphere, to Archimedes, Michelangelo, Galileo,

[1] *V.* experiment dated April 27, 1841, on the cutting of the vagus nerve in the rabbit (v, 410).

Descartes, and so on. Louis XIV compared himself to the sun. I am much more humble. I compare myself to a scavenger; with my hook in my hand and my pack on my back I go about the domain of science picking up what I can find."[1]

Longet claimed that he had taken part in the discovery of 1839, and made the claim public in medical journals; but when he came to perform the experiments by himself he was unable to find any trace of sensitivity in the anterior roots and he attacked Magendie for his inconsistency, disregarding his own. He presented his results in competition for the Academy of Sciences prize in experimental physiology for 1841. Unfortunately for him, Magendie was a member of the jury of award before whom the experiments had to be demonstrated. Longet became anxious when only two of the committee came to see his experiments and even brought the matter of the delay before the Academy. Finally, in 1842, he received the prize, but it was stated that one member of the committee, presumably Magendie, had abstained from voting.

Bernard was not ready to dismiss so readily the evidence which he had seen with his own eyes in Magendie's courses of 1839. Nevertheless, when Bernard and Magendie together attempted experiments to refute Longet, they themselves, much to their amazement, were unable to demonstrate any sensitivity in the anterior roots. This was the stage which the controversy had reached by 1841. It was Bernard, working alone, who in the end found the solution. He says:

> After much reflection I finally remembered that in the first experiments of Magendie a certain time, an hour or

[1] iii, 12.

more, elapsed between the moment when the spinal canal was opened and that when the sensitivity of the anterior roots was tested. In short, the animal was prepared some time before the lecture and it was only during the lecture that it was examined for the properties of the roots. I noticed on the contrary that in the more recent experiments which were not made with an eye to lecture demonstration both Magendie and Longet had examined the anterior roots immediately after opening the vertebral canal. It was therefore a question of time of which no one had taken account.[1]

Again, shortly after Bernard had become Magendie's *préparateur*, he repeated the experiment by which, in 1824, Magendie had thought that he had shown that destruction of the olfactory nerve does not entirely do away with the sense of smell. About a month later, Bernard happened to bring to the laboratory at the Collège de France for dissection in the course the head of a woman who had died of consumption in the Hôtel-Dieu. On opening the skull he was very much surprised to find the complete absence of olfactory nerves. He immediately became curious as to whether the woman had lacked a sense of smell in her lifetime. He hunted out the address in the rue de la Friperie at which she had previously lived in order to cross-examine her

[1] xi, 521; cf. *Comp. rend. Acad. d. Sc.*, 25: 104, 1847. Also iii, 24–5; iv, 40, 250; vii, 70, 366; viii, 305; xi, 517. This was not the only occasion upon which Bernard defended Magendie from Longet's attacks. Magendie had observed rotation of the body of a rabbit towards the side opposite to that on which he had wounded the cerebellum. Longet repeated this experiment and found that his rabbits rotated towards the wounded side. Did not Magendie know his right hand from his left? Bernard came to the rescue. The area which these investigations were wounding was not an exceedingly small one. Magendie's results were obtained when one spot, Longet's when another was wounded (xv, 45). On another occasion he publicly reproached Longet for trying to rob Magendie of the credit for his part in the discovery of the functions of the dorsal and ventral roots of the spinal nerves and giving the honour entirely to Charles Bell.

acquaintances on the point. Evidence was brought forward to show that she had not been able to stand the odour of a pipe and that she had been a good cook. When Bernard addressed himself to the man with whom she had lived for four years without benefit of clergy, the latter reported that she had liked flowers, had put them up to her nose to smell them, and the only peculiarity which he had observed in her was a tendency to melancholy. Bernard seems to have been less concerned with the macabre humour of the situation than with a regret that he had not known the woman while she was alive so that his evidence in support of Magendie's contention would have been at first hand.[1]

Through Magendie Bernard began some work in 1842 under the direction of Gay-Lussac, the most famous chemist in France at that time and patron of his friend, M. Pelouze. Gay-Lussac thought that the conclusions of the German physiologist, Magnus, regarding the gaseous content of arterial and venous blood were incorrect and in order to prove his point, in collaboration with Magendie and with the technical assistance of Bernard, he installed the apparatus for a series of experiments in Magendie's laboratory. The work was never completed or published, but Bernard referred to some of the results in one of his subsequent lectures.[2]

In May, 1843, Bernard's careful working out of the

[1] v, 229, 231. We now make a distinction, which neither Magendie nor Bernard made, between odorous and irritating substances. Sensations from the former are conveyed by way of the first cranial or olfactory nerves, the latter by the fifth. The woman without olfactory nerves might have been able to detect tobacco smoke, although one would like to be assured that she had no visual evidence of the presence of the pipe when she was being tested; she would not, of course, have been able to detect the odour of flowers, but might have experienced pleasure from touching them.

[2] xiii, 25.

anatomy of the small chorda tympani nerve resulted in his first published paper.[1] This paper had a physiological point, it is true, but the success of the investigation depended upon his skill in dissection. We find in it a description of his method of cutting in a living animal the chorda tympani, that small nerve which runs over the inner surface of the membrane separating the outer from the middle ear. He inserts a special small hook, punctures the membrane, which cracks with a peculiar sound, turns the hook and draws it out. He remarks: "It is so easy to do that I cannot see how M. Guarini had to give up the operation on account of its difficulty."

He afterwards enjoyed telling how some of his fellow *internes* had come upon him at work on this paper and had asked him what he was doing. When he told them that he was writing a memoir on the chorda tympani they expressed both surprise and amusement that he should be writing a long paper on such a small nerve. The little story,[2] apocryphal or not, serves to remind us that Bernard was not strictly a contemporary of his fellow students. Although still only an *interne* he was thirty years old. Furthermore, it is a matter of special interest to experimental physiologists that Bernard's first scientific publication was not the working out of a "research programme," the formulation of which in these days is considered so necessary a preliminary for acquiring funds for the laboratory. "I must say that these experiments were made at first not on the chorda tympani," Bernard said in a footnote to this paper, "but in an attempt to find an anastomosis between

[1] *Ann. méd.-psychol.*, 1: 408–39, 1843.
[2] Flint, A., jr., *Am. J. M. Sc.*, 76: 161, 1878.

the pneumogastric and the facial nerves. By chance, in a dog operated on for this purpose, I tested its sense of taste and perceived a diminution on the side with the facial nerve cut. Seeking for the cause of this phenomenon I was led to experiment directly on the chorda tympani." This is as perfect an illustration of the way in which Claude Bernard's mind worked as one could wish to find. Although he had devised his experiment to test one hypothesis, he was able to grasp the significance and implications of phenomena which turned up in the course of his experimentation.[1]

In this same year, 1843, Bernard undertook a new line of investigation. He set himself to discover what happens to different foodstuffs in the course of nutrition.[2] This was indeed an ambitious programme, as it would have involved virtually the whole realm of physiology. He was led to the conviction that phenomena of nutrition can be settled only from the point of view of the chemist. He started with digestion in the stomach, particularly the role of gastric juice. His original contribution was that gastric juice acts upon cane sugar, for he found that if sugar was injected into the blood after it had been acted upon by gastric juice it could no longer be recovered in the urine, whereas if cane sugar alone was injected into the blood it was excreted unchanged. He used these results for the thesis[3] for his degree of doctor of medicine which he presented in December of this year, throwing in for good measure a few pages on such unrelated topics as hydrocephalus, aneurysms and cupping glasses. He was successful in his defence of this thesis and in publishing it he dedicated it to

[1] For further discussion of this paper see Part II.
[2] xvii, 40; viii, 286.
[3] *Du suc gastrique et de son rôle dans la nutrition*, Paris, 1843. Cf. xiv, 249.

Magendie. The completion of his medical course did not fundamentally change his mode of life, except to transfer his activities completely from the hospitals to the laboratory. He never at any time engaged in the practice of medicine.

CHAPTER THREE

CIRCUMSTANCES OF THE EARLY YEARS OF SCIENTIFIC INVESTIGATION

La jeunesse, de tous côtés, se lance avec ardeur dans les sciences modernes qui ont pour objet d'analyser les phénomènes de la vie et d'en déterminer les conditions à l'état physiologique ou pathologique.[1]

HAVING GOT his medical degree out of the way, Bernard was free to devote himself altogether to his investigations and these immediately began to give rise to the publications which followed one another in rapid succession from 1844 on. His paper on the spinal accessory nerve[2] was the natural outcome of his experiments while still an *interne* on controversial points raised by Longet against Magendie. The paper was published in April, 1844, and there is a ring of triumph in the author's use after his name of his new title, Doctor of Medicine, along with the descriptive phrases, "former *interne* and *préparateur* for the course in physiology at the Collège de France." When the paper was republished in an extended form in 1851 by the Academy of Sciences all these explanations of who the author was had been discarded and it was signed simply "Claude Bernard." In the meantime the paper had been submitted for the prize in experimental physiology offered by the Academy of Sciences for 1845. It was not until December 21, 1846, that arrangements were made for a jury (of which Flourens was chairman and Magendie a

[1] xi, 408.

[2] *Arch. gén. de méd.*, 4: 397–424; 5: 51–96, 1844. Cf. Part II.

member) to see a demonstration of the dissections and experiments involved. The notice of the appointment for this demonstration was preserved by Bernard and is still to be found among the private papers in the possession of his great-nephew, M. Devay. The award to him was announced in April, 1847, when the jury complimented him on the accomplishment of a task "as difficult as it was important."

In 1844 Bernard began his collaboration with the young chemist, Barreswill.[1] Barreswill had just published a paper on the chemical constitution of certain metallic salts of organic acids and had carried out his experiments in the laboratory of another assistant at the Collège de France, M. Paul Thénard, who performed the same duties for the professor of chemistry that Bernard did for the professor of medicine. It was Barreswill's copper tartrate compound which was presently to stand Bernard in such good stead in his studies on sugar in the living organism. The two friends continued to collaborate for about five years. They published six papers together and in several of his own papers on chemical aspects of nutrition during this period Bernard stated that the work had been done in collaboration with M. Barreswill.[2] Bernard must have been very willing to co-operate with others for we find in 1844 M. Melsens, who was working on gastric acidity, thanking Bernard for his kindness in putting at his disposal a dog with a gastric fistula. The list of names actually appearing on papers with Bernard's includes Davaine, Rayer, Brown-Séquard, Pelouze, Charcot and Robin, the last joint paper

[1] Spelled Barresvil, Berriswill and Barriswill as well as Barreswill in the *Comptes rendus* of the Academy of Sciences. Bernard at this time had in parenthesis after his name "de Villefranche" to distinguish him from Paul Bernard, a surgeon, whose work, that, for example, on the lacrimal glands, might have been confused with his.

[2] *Comp. rend. Acad. d. Sc.*, 31: 371–4, 1850.

being published in 1854, the year before Bernard succeeded to Magendie's chair.

The paper with M. Pelouze was the outcome of an incident of 1844 when he had presented Bernard with some arrows from South America tipped with the peculiar native poison, curare. The investigation of the properties of this poison, carried on over a long period of years, was one of the most fruitful of Bernard's career. M. Pelouze's great service to his friend, however, was the provision of a place in his laboratory where Bernard could carry out experiments demanding the use of chemical apparatus. There are frequent references in Bernard's writings to his indebtedness to M. Pelouze in this respect between the years 1844 and 1848. M. Pelouze was in a position to understand the difficulties under which Bernard struggled. He had himself started out as a pharmacist's apprentice, and when a medical student had served an interneship under the irascible Magendie. He was, however, six years older than Bernard and had got launched on his medical career a little earlier. He was therefore ready to furnish a badly needed asylum for the experimental beginnings of Bernard's greatest discoveries, especially the glycogenic function of the liver. The parallelism between the careers of the two friends was eventually completed when both attained professorships at the Collège de France.

There is no paper recording collaboration between Magendie and Bernard, but Magendie in his lectures frequently referred to Bernard as his collaborator.[1] Bernard himself records working with Magendie in 1844 on the differences in temperature in the blood of the two sides of the heart.[2] As president of a

[1] *Union méd.*, 6: 2, 1852. [2] vi, 57; xiii, 42.

commission on "equine hygiene" appointed by the Minister of War, Magendie had at his disposal a great number of horses destined for the slaughter-house, some of which he decided to use for experiments on animal heat. Bernard performed the difficult operation of inserting two long thermometers into the heart of the living horse, one by way of the carotid artery, the other by way of the jugular vein. The veterinary school and the abattoirs at Alfort on the outskirts of Paris often served as laboratory when large animals were needed for experimentation.[1] Renan, after Bernard's death, told a story belonging to this period which had evidently become a legend. He said:

> These experiments [at Alfort] on mad horses, on creatures infected with every kind of virus, were sometimes dreadful. Dr. Rayer had just discovered that the most terrible disease in horses is transmissible to the human beings who attend them. Bernard wished to study the nature of this hideous disease. In a violent convulsion the horse tore the back of his hand and covered it with saliva. "Wash quickly," said Rayer, who was beside him. "No, don't wash, you will hasten the absorption of the virus," advised Magendie. There was an instant's hesitation. "I'll wash," said Bernard, putting his hand under the tap, "it's cleaner."[2]

This anecdote, it may be remarked in passing, has the flavour characteristic of an alliance between biography and rhetoric. It has a moral, it is highly circumstantial, and perhaps, a little unconvincing.

Van Tieghem claims that at this period Magendie "supported with impatience the superiority of his pupil" and went to the length of forbidding Bernard to work for himself in the laboratory at the Collège de France. Up to 1840 Magendie had had as a

[1] vii, 114, 186.   [2] xix, 25.

laboratory only a very small room which Bernard described as a closet which could scarcely contain the two of them.[1] It was with the greatest difficulty that in that year Magendie was able to overcome the obstructive tactics of the administration and fit up a corner in the Collège de France for a laboratory, and here he was inclined to regard everything as for the master only.

The business of setting up a private laboratory for animal experimentation in those days had its attendant difficulties, and it is in the course of a description of these difficulties that Bernard acquaints us with the date of the establishment of his own laboratory a stone's throw from the medical school. He said in 1867:

> Twenty-five years ago when I began my career in experimental physiology, I found myself in those difficulties which are reserved for experimenters. . . . As soon as an experimental physiologist was discovered, he was denounced, became the abomination of the neighbours, and was handed over to the police for prosecution. At the beginning of my experimental studies I ran into such difficulties many times; but I must admit that a stroke of chance in my case brought it about that I should actually come under the protection of a police-commissioner. . . . It was about 1844; I was studying the properties of gastric juice with the aid of a discovery of M. Blondlot which consisted in collecting the gastric juice by means of a cannula or a sort of silver faucet inserted into the stomachs of living dogs, a proceeding which caused no detriment whatever to their general health. A celebrated surgeon from Berlin, Dieffenbach, came to Paris; he heard of my experiments through my friend, M. Pelouze, and wished to see the cannula operation done. Having been told of his wish, I hastened to satisfy it, and I performed the

[1] xv, 63; cf. ix, 235, n. 232.

experiment on a dog in the chemical laboratory of M. Pelouze in the rue Dauphine. After the operation the animal was shut up in the court so that we could observe him later. But the next day the dog got away in spite of our watchfulness, carrying with him in his stomach the accusing cannula of a physiologist. Some days later, early in the morning, when I was still in bed, I received a visit from a man who came to tell me that the police-commissioner of the Medical School quarter wanted to speak to me and I was to go to his house. I went some time during the day to the house of the police-commissioner in the rue Jardinet. I found a little old man, very respectable-looking, who received me at first very coldly and without saying a word, then, taking me into another room, he showed me to my great astonishment, the dog I had operated on in the laboratory of M. Pelouze, and asked if I could tell him who had put that instrument into the dog's stomach. I replied that I could, adding that I was very happy indeed to find my cannula which I had thought lost. My confession, far from satisfying the commissioner, at first provoked his wrath, for he admonished me with exaggerated severity, accompanied by threats, for having had the audacity to take his dog for experimentation. I explained to him that it was not I who had taken his dog, but I had bought it from persons who sold dogs to physiologists and who said that they were employed by the police to collect stray animals. I added that I regretted having been the involuntary cause of the pain which the misadventure to his dog had caused him; but the animal would not die as a result of what had happened to it; there was only one thing to be done, to let me take back my silver cannula and him keep the dog. These last words caused the commissioner to change his tone, and his wife and daughter now appeared appeased. I removed my instrument and promised to return later. I did in fact return many times to the rue Jardinet. The dog was perfectly cured within a few days; I became the friend of the commissioner and I found that I could count hence-

> forth on his protection. That is why I came to install my laboratory in his district, and for many years I was able to continue my private courses in experimental physiology in that quarter under the protection of the police-commissioner, right up to the time when I was nominated substitute for Magendie at the Collège de France.[1]

The laboratory to which Bernard refers in this story was a small mezzanine room at 5, Commerce Saint-André-des-Arts, in the very centre of the Latin Quarter. It is plain that he was obliged to supplement the income which he derived from assisting in Magendie's course at the Collège de France, but this laboratory was used not only for the private courses of which he speaks but also for his experimental investigations. He says in a letter to M. Barral that here he often dreamed over his experiments and hit upon the solution of knotty problems which had preoccupied his mind. He grew very fond of the place and even became interested in its historical associations.

The actual building is now gone, but the Commerce, taken with the passages opening from it, still makes a fascinating spot for lovers of old Paris. It is a passage lying between the Boulevard Saint-Germain, close to the École de Médecine, and the rue Saint-André-des-Arts. Bernard told M. Barral that it had originated as a sunken ditch for the defence of the Porte de Buci in 1582. Even in his time there had existed at number 8 a much-frequented reading-room founded at the time of the Convention by the widow of Brissot, who had taken an assumed name in order to use her husband's library as a stock-in-trade after his execution. In the same house was the printing-press of the *Ami du Peuple* which Marat had established

[1] ix, 144–6.

there under a requisition from the Commune. Directly opposite Bernard's laboratory was the place where Dr. Guillotin, whose house faced both on the Commerce and the rue de l'Ancienne Comédie, tried out his deadly instrument on sheep, creating a sort of precedent in the neighbourhood for Bernard's animal sacrifices in the cause of science. Further along was the opening to the triple cour de Rohan which joins what is left of the rue du Jardinet, where ~~the~~ police-commissioner lived, with the Commerce. In the first court was the base of a tower belonging to the fortifications of Philip Augustus, on the top of which in the summer flowers blossomed in boxes. Little girls from the school near by romped over the stones in their recess time. In the summer of 1935 the remains of the old fortifications were still discernible and they were gay with bright geraniums.

It was still in the year 1844 that Bernard presented and defended a thesis on coloured substances in the human body[1] in competition for the position of *agrégé* (assistant professor) in the section of anatomy and physiology of the Faculty of Medicine. Of the six members of the committee of selection only one, M. Blandin, a friend of Magendie, voted for Bernard. He had failed; but van Tieghem says his failure was due to his not having "the qualities of speech which could make him succeed in this type of competition. He appeared awkward and embarrassed. Then, too, his judges and competitors scarcely predicted for him anything more than the most modest of medical careers." Another of Bernard's biographers, M. Faure, can only approve of the committee's decision, for he knew personally two of the successful candidates, Béclard and Sappey, both of whom became

[1] *Des matières colorantes chez l'homme*, Paris, 1844.

professors in the Faculty of Medicine, the first in physiology, the second in anatomy. In Faure's opinion the best candidates were selected. It is true that Bernard never excelled as an academic lecturer. He was an investigator, and it is really fortunate that for a second time he received a check like that administered by M. Saint-Marc Girardin ten years before to put him on his right way.

In 1845 he suffered another disappointment when he made application for membership in the Academy of Medicine in the section of anatomy and physiology at the death of M. Edwards. His name did not even appear in the list of nominees, and M. Longet was elected.

Little has been said by Bernard's biographers of his domestic life. If it were not for a few brief references to an unhappy home, one might think that he had remained a celibate and had had only science for a mistress. As a matter of fact, when Bernard was thirty-two, a marriage was arranged between him and Marie Françoise Martin, who was then twenty-six and the daughter of Henri Martin, a physician living on the Boulevard Saint-Denis in Paris. Bernard at this time was living in the rue du Pont de Lodi, not far from his laboratory. M. Pelouze later admitted responsibility for having brought this marriage about, partly in the hope of keeping Bernard in Paris when he was discouraged and tempted to abandon his scientific career for a country practice at Saint-Julien, and he was grateful to Bernard, when things turned out badly, for not having held it against him. His name appears in the list of Bernard's sponsors in the marriage contract along with those of Magendie and Madame Magendie. This contract gives us a rather pathetic enumeration of the worldly goods which

CLAUDE BERNARD'S BIRTHPLACE

BERNARD'S HOUSE AMONG THE VINEYARDS ON CHATENAY, OVERLOOKING SAINT-JULIEN

PLATE II

Bernard brought to the marriage—some furniture, linen, pots and pans, his library, his personal wardrobe and cash on hand, the whole adding up to a value of 9,800 francs or something less than £400. The bride's dowry was officially 60,000 francs, and in her case clothes worth 6,000 francs were not included. Because the dowry was not paid in cash at once the bride was to receive a yearly income of 5,000 francs arising partly from interest and partly from rents. The first child of this marriage, a son, Louis Henri, was born a year later, May 8, 1846, but died when he was only three months old. Two daughters were born, the first, Jeanne Henriette, always called "Tony,"[1] August 20, 1847; the second, Marie Louise Alphonsine, May 14, 1850. These daughters survived Bernard by many years but he had no surviving sons. A second son, Claude François Henri, born January 31, 1856, lived fourteen months.

There are indications that Bernard attempted to augment his meagre income at the time of his marriage. He prepared anatomical dissections in 1845 to be drawn by H. Jacob for the anatomical atlas which the latter published with Bourgery in a series of volumes extending from 1832 to 1854. He collaborated with M. Ch. Huette on an illustrated textbook of surgery. This book was reprinted as late as 1873 and translations into English, German, Dutch, Italian and Spanish have been made. The English translation met with favour, as is shown by the fact that this book was given to each surgeon in the United States Army during the Civil War. Bernard also tried to establish a laboratory with his friend,

[1] On the stone over the family grave the daughters are designated "Tony Claude" and "Marie Claude."

Dr. Lasèque, in the rue Saint-Jaques, just behind the Collège de France, near the Panthéon. They had five or six pupils, but the venture was not a success; the tuition of the pupils did not pay for the rent of the shed and the cost of the rabbits.[1]

Bernard was also at this time working at his experiments every available moment of the day, even upon occasion impersonating the experimental animal. Here is the time-table of one day:

> On the 6th of June, 1846, I submitted myself to a diet with as restricted a nitrogen content as possible in order to vary the reaction of my urine. The night before, I had eaten for dinner pigeon, green peas, milk soup, cheese and coffee. Urine was acid. . . . On the 6th at 8 a.m. I ate two large plates of potato starch boiled with water and butter. Urine examined at 10 and 11.45 was acid. . . . At noon I ate cauliflower with an oil dressing, carrots fried in butter, lettuce salad, wine and sugar, but nò bread. Urine at 2 and 4 p.m. acid. . . . At 6 a.m. I ate two plates of soup with vegetables, fried potatoes, buttered peas, boiled potatoes instead of bread, a salad of oranges in brandy and coffee. Urine acid at 10 p.m. . . . To bed at midnight, urine less pronouncedly acid. . . . Rose at 6 next day, urine alkaline. Breakfasted at 8 a.m. on *café au lait* and bread.[2]

An obliging cook seems to be implied in the background.

The subject engaging his attention throughout this year was digestion and nutrition. He was particularly interested in the differences of nutrition of herbivores and carnivores, and in the account of the day's menu just quoted was, of course, temporarily turning himself into a herbivore. Some of the experiments

[1] Renan (xix, 21) is mistaken in saying that it was here that Bernard conceived the idea of his experiments on the chorda tympani and gastric juice; the results of these experiments had been published in 1844, two years before this laboratory was established.

[2] ii, 460.

which he was currently engaged upon were crucial for one of his major discoveries, the role of the pancreatic juice in the absorption of fat, of which the definitive account did not appear until early in 1849.[1] Bernard realized that these early investigations illustrated his characteristic method of approach to physiological problems and he has preserved for us a detailed account of this particular discovery. He says:

> One day, rabbits from the market were brought into my laboratory. They were put upon the table where they urinated, and I happened to observe that their urine was clear and acid. This fact struck me, because rabbits, which are herbivores, generally have turbid and alkaline urine; while on the other hand carnivores have clear and acid urine. These observations of acidity in the rabbits' urine gave me an idea that these animals must be in the nutritional state of carnivores. I assumed that they had probably not eaten for a long time, and that they had been transformed by fasting into veritable carnivorous animals, living on their own blood. Nothing was easier than to verify this preconceived idea or hypothesis by experiment. I gave the rabbits grass to eat; and a few hours later, their urine became turbid and alkaline. I then subjected them to fasting and after twenty-four hours, or thirty-six hours at most, their urine again became clear and strongly acid; then after eating grass, their urine became alkaline again, etc. I repeated this very simple experiment a great many times, and always with the same result.

He varied this experiment by feeding rabbits cold boiled beef to change them into carnivores. When he autopsied these animals to see if the beef had been digested, he happened to notice that the

> . . . white and milky lymphatics were first visible in the

[1] ii, 180: "It was in 1846 that we made this observation [as to the point in the intestine where absorption of fat takes place], and it was not until two years later that we published the result of our experiments."

> small intestine at the lower part of the duodenum, about thirty centimetres below the pylorus. This fact caught my attention because in dogs they are first visible much higher in the duodenum just below the pylorus. On looking more closely, I noted that this peculiarity in rabbits coincided with the position of the pancreatic duct which was inserted very low and near the exact place where the lymphatics began to contain a chyle made white and milky by emulsion of fatty nutritive materials. Chance observation of this fact gave me an idea and brought to birth in my mind the thought that pancreatic juice might well cause the emulsion of fatty materials and consequently their absorption by the lymphatic vessels. Instinctively again, I made the following syllogism: the white chyle is due to emulsion of the fat; now in rabbits white chyle is formed at the level where pancreatic juice is poured into the intestine; therefore it is pancreatic juice that makes the emulsion of fat and forms the white chyle.

This had to be decided by experiment,[1] and Bernard proceeded to devise and perform a suitable experiment to verify the truth or falsity of his suppositions.

Like Descartes, who experienced the "revelation" of his method early in his career, Bernard also clearly envisaged the principles of the method which eventually he was to develop into a canon in his famous *Introduction to the Study of Experimental Medicine*; first, the chance observation, then logical construction of an hypothesis based upon the observation, finally, the experimental verification of the hypothesis. By 1846 he was well on his way. It was in the first ten years of his experimental work that he accomplished the major part of his original contribution to physiology.

Bernard's first academic advancement came in 1847 when he was appointed *suppléant* or substitute

[1] viii, 267, 269. Cf. Part II.

lecturer to Magendie at the Collège de France. From now on Magendie gave the winter lectures beginning in December and Bernard the summer lectures beginning in May. Bernard's opening lecture was an echo of Magendie who, when Bernard attended his course as a student, had said: "The medicine I am to teach you is a science in the making." Bernard went even further and said: "The scientific medicine which it is my duty to teach you does not exist. The only thing to do is to lay the foundations upon which future generations may build, to create the physiology upon which this science may later be established."[1] The type of lecture demonstration which Magendie had made customary at the Collège de France gave Bernard the opportunity of appearing at his best. M. Fauconneau-Dufresne in reporting the course for 1853[2] said: "Every one knows with what eagerness pupils flock to this young and eminent professor."

The first scientific society in which Bernard was elected to membership was the Société philomathique de Paris. His election took place January 16, 1847. This society had been founded in 1788 to keep its members abreast of the latest scientific discoveries. In the language of its manifesto it took "for inspiration the whole panorama of the progress of the human spirit." Its membership in 1847 included the names of many scientists who were associated with Bernard in other ways. Magendie had been a member since 1813; Dumas and Boussingault, whose theories on the differences in metabolism of plants and animals were to influence Bernard in his quest for glycogen, were also members of some years' standing; de Quatrefages, elected in 1847, and Longet, in 1845, were to be rival candidates with Bernard for a seat in the

[1] xi, 456. [2] *Union méd.*, 7: 297, 1853.

Academy of Sciences; Berthelot, the chemist, Milne-Edwards and Brown-Séquard, the physiologists, were also members. During the first two years of his membership Bernard contributed some half-dozen papers.

Beginning with 1849, however, he seems to have found the Société de Biologie a more satisfactory outlet for his communications. He was one among a number of the younger physicians and naturalists who had met in May, 1848, to form a society for the study of "phenomena pertaining to the science of life." It was the era of the positivist philosophy, the influence of which was felt and acknowledged by the original members. Berthelot, Bernard's colleague at the Collège de France and one of the early members, said of the Société de Biologie: "It was from the very first and continued to be a powerful centre of scientific initiative, more alive and more liberal than the academies." He added that Claude Bernard was its "shining star and favourite." The first president was M. Rayer, a close friend of Bernard and, later, his personal physician. For the first two years of the society's existence Bernard was one of its two vice-presidents. The first year in which the records of the society contain no paper by him was 1861. At the death of M. Rayer, November 9, 1867, Bernard was elected perpetual president, an honour which he held until his own death, when he was succeeded by his pupil, Paul Bert.

Upon looking over the list of communications we find that Bernard was extremely active. The members met every week and the records of almost every session include a reference to him. At the first meeting, on January 6, he took part in the discussion following a demonstration of skulls of women who had died in childbirth. On January 13 occurred his

first communication; he reported on the presence of sugar in the vomitus of a diabetic who had taken only meat. On January 20 he reported on experiments with atropine. On January 27 he reported not only on movements of the head of animals after puncture of the fourth ventricle of the brain, but also on a hen which laid double-yolked eggs; it seems that, when hatched, these eggs gave rise to nine pairs of twins, eighteen perfectly formed chicks from nine eggs, one egg having failed to hatch. Succeeding months saw him equally active. It was, indeed, before the Société de Biologie that he presented in detail and in the order of their accomplishment the discoveries upon which his great reputation finally rested.

In this first year of the Société he was working most intensively on the subject which first engaged his attention in 1846, the rôle of the pancreas in digestion. He read memoirs on this topic not only before the Société de Biologie, but also before the Academy of Sciences, and for this work he received for the second time, in 1850, their prize in experimental physiology.

It was in recognition of this work that the red ribbon of the Legion of Honour was conferred upon Bernard in April, 1849, and the part which Magendie played in the affair was afterwards revealed in an anecdote told by one of Magendie's former pupils, Dr. Constantin James. Magendie was most enthusiastic over Bernard's discovery, and after the paper was read before the Academy, realizing that Bernard himself would never make overtures to the authorities for public recognition, he took the matter in his own hands. He went to the Minister of Public Instruction who at once agreed to the nomination of Bernard as Chevalier of the Legion of Honour. Magendie was

delighted and asked that Bernard be kept in ignorance of his new honour until the affair was actually accomplished in order that he might enjoy the surprise. When, two days later, the notice appeared in the *Moniteur*, Dr. James went to proffer his congratulations and found, as he expected, that Bernard had not yet read the announcement. Enlightened, Bernard expressed himself as very pleased and added:

"I am sure that it is to Magendie that I owe this!"

Dr. James went on to point out that Bernard had been decorated, not as a physiologist, but as a musician, and he pretended to read what follows from the paper as if it really were there:

"Claude Bernard (of Villefranche) is named Chevalier of the Legion of Honour for his excellent work on the play of the pancreas, the harmony of its functions, its perfect accord with the liver, the concert which it . . ."

Bernard interrupted the reading. "What is all this? Give me the paper and let me read it for myself."

He read: "Claude Bernard (of Villefranche) is named Chevalier of the Legion of Honour for his excellent work on the *musical* properties of the pancreas."

Bernard was quite upset and protested, "Musical for medical! What a stupid blunder! How can we get this silly mistake rectified?"

The necessary correction was, of course, made in a subsequent issue of the *Moniteur*.[1]

In 1848 Bernard had begun experiments on the liver in M. Pelouze's laboratory in collaboration with M. Barreswill, experiments which in the course of time led to what is generally considered to be his greatest discovery, the glycogenic function of the liver. These experiments were a sequel to his work on digestion

[1] Genty, M., *Biogr. méd.*, p. 140 (Sept.) 1932.

and showed that in dogs on normal diets and even dogs fed on meat alone sugar was present in the liver. The first report was a joint paper read before the Academy of Sciences in August, 1848,[1] and the authors proudly exhibited a vial of alcohol which had been produced by the fermentation of this new liver sugar with brewer's yeast. As the work progressed additional papers appeared under Bernard's signature alone. In 1850 he submitted before the Academy an account of his work up to date entitled "A new function of the liver in man and animals,"[2] and for this work he was in 1851 for the third time awarded the Academy's prize in experimental physiology, Magendie, as on the former occasions, being a member of the committee. Bernard used the same material for the thesis which he defended, March 17, 1853, to obtain the degree of Doctor of Natural Sciences.[3]

In 1849 Berhard announced what from a physiological point of view is his most spectacular discovery, that a slight wound in the floor of the brain near the cerebellum renders an animal temporarily diabetic.[4] He was often asked how he came to make this singular discovery, whether it was a matter of mere chance, for at first he reported only the results of his experiment and not the reasons which had prompted him to make it. The point was that he had started out on an hypothesis which led to the desired result, but which was nevertheless false. He had previously found that cutting the vagus nerves interrupted the secretion of sugar from the liver and he now wished, if possible, to produce the opposite effect, viz., an

[1] *Comp. rend. Acad. d. Sc.*, 27: 249; 253; 514, 1848.
[2] *Comp. rend. Acad. d. Sc.*, 31: 371–4, 1850.
[3] *Recherches sur une nouvelle fonction du foie considéré comme organe producteur de matière sucrée chez l'homme et chez les animaux*, Paris, 97 pp.
[4] *Mém. Soc. de biol.*, 1 (*C.R.*): 14, 1849.

increase in the output of sugar. It is a general physiological principle that if cutting a nerve abolishes a given phenomenon artificial stimulation of the nerve will produce it. He therefore tried stimulation of the vagi but there was no increase in the output of sugar by the liver. Then he remembered that in cases where artificial stimulation of a nerve had been without effect a wound in the brain at the point where the nerve emerged was often effective. Consequently he now tried puncturing the brain near the emergence of the vagus nerves. The very first rabbit upon which he operated gave the desired result, for sugar appeared in the urine in abundance; but, unfortunately, the next eight or ten operations gave absolutely negative results. He was puzzled. Had it not been for the first positive result he would have concluded that wounding the brain was as ineffective in increasing the secretion of sugar from the liver as stimulation of the vagus nerves had been. But the one positive result was an observed fact which he could not dismiss. It turned out that the area of the brain which must be punctured is a small one and, by chance, he had hit it in the very first animal in this series of experiments and in the first one only. Then came the disturbing discovery that if he cut the vagi between the brain and the liver before he punctured the brain, the diabetic condition resulted just the same as if the vagi were intact. Therefore the vagi were in no way responsible for his results and his original hypothesis had been entirely false. He later found that it was the sympathetic nerves rather than the vagi that were responsible for the phenomenon.[1]

Another line of inquiry was occupying his attention by 1851, the influence of the sympathetic nerve on

[1] i, 323; viii, 304. Cf. Part II.

temperature. He announced before the Société de Biologie towards the end of that year that cutting one sympathetic nerve brought about a rise of temperature on the same side of the face. This observation led in the end to the discovery of the vasomotor system, but it was apparently not until 1852 that Bernard grasped the real significance of his experiments, for he always himself dates this discovery from the paper read before the Academy of Sciences, March 29, 1852.[1]

Two other physiologists, Budge and Waller, received the prize of the Academy of Sciences in experimental physiology for 1852 for their work on the sympathetic nerve. When Bernard announced his discovery they claimed that their work had implied it. This at once roused Bernard to a spirited defence of his own work. He replied on the spot:

> I do not agree with M. Budge in his long discussion which is entirely foreign to the point of my communication of March 7. I have established in my researches on the cephalic portion of the sympathetic nerve that, in addition to the pupillary phenomenon, there is heat production and other phenomena as well. This I proved in the historical survey in my note, for the absolute accuracy of which in every point I will vouch.

Bernard's defiance of Budge and Waller was vindicated when the committee of the Academy of Sciences, headed by Magendie and including Rayer, Fluorens, Serres and Milne-Edwards, at the beginning of 1854 awarded him the Academy's prize in experimental physiology for 1853 for his work on the sympathetic nerve. This was the fourth and last time that Bernard received the prize, for presently, on his election to the Academy of Sciences, he was to become himself the

[1] *Comp. rend. Acad. d. Sc.*, 34: 472–5, 1852. Cf. xiii, 207.

chairman of the committee annually awarding the prize.

By 1850 Bernard's fame as an exponent of the experimental method in medicine began to be spread throughout Europe and overseas to the United States of America.[1] At this time young physicians in America, as soon as they had taken their medical degrees, set out, if they were able, on the *grand tour*, and their principal objective was France rather than Germany. It is said that not only did they visit hospitals, their primary interest, but nearly all of them attended some of Bernard's lectures at the Collège de France, and a few engaged in experimentation under his direction. One of the best-known American doctors of the nineteenth century, Weir Mitchell of Philadelphia, spent the year 1850–1 in Paris, and although he attended courses designed for surgical training, he wrote home that he liked Bernard's lectures in physiology and Robin's in microscopy much better. Bernard's pedagogical attitude is well illustrated in a conversation with Weir Mitchell. The latter had said that he thought so and so must be the case. "Why think," replied Bernard, "when you can experiment? Exhaust experiment and then think." It might almost have been Magendie who was speaking. When Mitchell returned to the United States he continued experimentation on poisons, and Bernard quoted his results on rattlesnake poison in a lecture in 1860.[2]

Another American pupil, John Call Dalton, is said after his return to have been the first to teach physiology in the United States using living material as he had seen Bernard do; and his textbook on physiology,

[1] Olmsted, J. M. D., *Calif. & West. Med.*, 42: 111–13; 174–6, 1935.
[2] xi, 150.

each edition of which was kept up to date on Bernard's discoveries, was the most widely used of all textbooks on this subject in the United States in the 1880's.

From England in 1852 came Dr. John Scott Burdon-Sanderson, who was destined to become professor of physiology, first at London University, then at Oxford. Bernard set him the task of collecting pancreatic juice from a rabbit. Burdon-Sanderson made several unsuccessful attempts on both rabbits and dogs before he learned the trick of inserting the cannulas. F. W. Pavy, another young English physician in Paris at the same time, became, after leaving Bernard's laboratory, one of the foremost authorities on diabetes, and was rash enough to differ with the master on certain points regarding glycogen formation. So great was the impression which Bernard had made upon him that he is said to have copied Bernard's manner as well as his matter when he came to give his own lectures in London.

There are many references in Bernard's lectures to the work of his pupils, but the names are mostly French. There are a few Italians, e.g., Signor Vella of Turin,[1] an occasional Russian,[2] and very few Germans, e.g., Kunde.[3] His most distinguished German pupil, Willy Kuhne, did not come to the laboratory until the end of the 50's.[4] Ludwig, who came to have the reputation of being the most successful of all the teachers of physiology in Europe, soon began to attract not only the Germans but also English and Americans, so that a little later Bernard's group was almost entirely French.

Bernard's growing prestige made membership in the Academy of Sciences inevitable, but election to it proved a definitely more difficult matter than to the

[1] v, 148. [2] x, 356. [3] iv, 141. [4] xi, 232.

newly organized Société de Biologie. The Academy had been founded in 1666 and is one of the five Academies composing the Institute of France. The ordinary membership is limited to sixty-six and vacancies occur only through death. In 1850 M. de Blainville, a member of the section of anatomy and zoology, died and Bernard applied for his seat. The nominating committee placed MM. Coste and Quatrefages first on the list as of equal rank and M. Coste was elected. Again in 1852 there was a vacancy through the death of M. Savigny. This time the report of the nominating committee, which as usual held its session in private, was as follows: first on the list as equals were presented the names of MM. Dujardin and Quatrefages, then followed in order of preference three others with no mention of Bernard. One of the members of the committee proposed to add the names of two physiologists, Claude Bernard and M. Longet. This was done, but M. Quatrefages was chosen. In 1854 the death of M. Roux, the celebrated surgeon, of the section of medicine and surgery, created another vacancy. Bernard again applied. This time the chairman of the nominating committee was Magendie, and on the list of candidates selected by the committee Bernard's name came first. M. Longet applied earlier but withdrew, which was perhaps just as well if Magendie was pushing Bernard for the election. At the next meeting of the Academy, of the 51 votes cast Bernard received 42 and was elected. On July 3rd the Minister of Public Instruction sent to the Academy a duplicate of an Imperial decree dated July 1, 1854, which approved the election of M. Claude Bernard, and at the age of forty-one he took his seat among his confrères.

CHAPTER FOUR

PROFESSORIAL ACTIVITIES

Nous nous préoccuperons sans cesse, non de ce qui est fait, mais de ce qui reste à faire.[1]

THE YEAR 1854 was an important one for Bernard in another respect. One of the two chairs of botany of the Faculty of Sciences at the Sorbonne became vacant. Through the influence of Bernard's friend, M. Rayer, this chair was suppressed; in its place was created a new chair of general physiology, and this was given to Bernard. Although the title of general physiology was adopted the course dealt with animal physiology only.

M. van Tieghem attended this course in 1861 and reports that the lectures were merely didactic and entirely without experimental demonstration. Furthermore, Bernard appeared timid, embarrassed, and ill at ease. He was, in fact, "a mediocre lecturer," sums up van Tieghem, "but the importance of the problems raised was so great, so sure was the method applied to resolve them, and so elegant the solution that soon one no longer thought of the lecturer's unfortunate manner." This somewhat left-handed praise may have been prompted by van Tieghem's feelings regarding the suppression of the chair of botany. He was himself a botanist and it was his master, M. Pierre Duchartre, who was disappointed in his legitimate expectation to succeed to the chair of botany at the Sorbonne when the new chair was created for Bernard.

[1] i, 11.

Van Tieghem in his biographical note on M. Duchartre[1] makes caustic comments on the reasons advanced at the time by the Minister of Public Instruction for the suppression of the old chair and he describes the effect on the Faculty of Sciences at the Sorbonne as a mutilation from which it had not recovered at the end of half a century. Bernard's own conception of what was expected of him in his new post at the Sorbonne is expressed in a lecture which he gave at the Collège de France in December of the same year at the beginning of the course in which he was still acting as substitute lecturer for Magendie. He said:

> At the Collège de France a professor always preserves the point of view of an explorer and must think of science not in relation to what she has already accomplished and established, but in relation to the gaps which are still left, in order that they may be filled in by new researches. It is therefore the most difficult and obscure questions which he chooses to attack before an audience prepared by previous studies to attempt them. In the University Faculties, on the contrary, the professor takes up a dogmatic position and attempts to unite in a synthetic exposition the entire range of scientific facts, linking these facts together by means of those bonds which we call theories, and these theories are designed to gloss over as much as possible obscure and controversial points which would profitlessly cloud the mind of a beginner.[2]

There are indications that Bernard tried to carry out this general plan in the only set of his lectures at the Sorbonne which are included in the *Leçons* published in book form, those on "The Properties of Living Tissues" given in 1864. These lectures are theoretical and descriptive, with only a very occasional

[1] *Mém. Acad. d. Sc.*, 51, i, 1910. [2] i, 10.

demonstration. He seems to have been very conscious, however, that this was not a satisfactory method of teaching for himself. For the first ten years of his professorship at the Sorbonne he exerted every effort, without success, to obtain in connection with it a laboratory and a laboratory assistant in order that he might give his course the experimental development which he thought it demanded. It was only in 1864 that we find him referring to Paul Bert as his *préparateur* at the Sorbonne.[1] In the end, his lectures there became a repetition of the material which he used in those given at the Collège de France.[2]

On October 7, 1855, Magendie died at the age of seventy-two. In Bernard's last interview with his old teacher Magendie said: "My chair will come to you; with you I know that it won't fall to a molly-coddle [*en quenouille*]." Two months after his death Bernard was appointed his successor as Professor of Medicine at the Collège de France.

His first lecture in this capacity was a eulogy of his predecessor. He pointed out that Magendie had given the chair at the Collège de France its peculiar character. He had realized that in order to make medicine scientific the methods of the exact sciences of physics and chemistry must be introduced into it, and therefore the course in medicine at the Collège de France became under him pure physiology.[3] Bernard, who had been brought up under this regime and owed so much to Magendie's teaching, set himself to carry on the tradition.

His great admiration for Magendie's qualities did not blind him to his weaknesses. Of the admiration

[1] x, 320; cf. ix, 235, n. 234.

[2] xix, 360, 362. M. Émile Alglave gave a résumé of all Bernard's lectures in his *Revue des cours scientifiques* after 1864, and in referring to those given at the Sorbonne he states several times that they were in great part repetitions of lectures already given at the Collège de France.

[3] iii, 23.

there is no doubt. He said: "Magendie having been my master I have the right to be proud of my scientific descent."[1] He particularly trusted Magendie's powers of observation. When Magendie stated that in certain cholera patients in the course of the severe epidemic of 1832 he had established the complete absence of all circulation although these patients moved and spoke and even sat up, Bernard did not hesitate to use this observation as an argument to prove that in man blood is not indispensable to life and to link up with this his theory that cold-blooded animals in hibernation do not need red blood cells for the carrying of oxygen since, as he claimed, no oxidation was going on under these conditions. In this case, of course, both Magendie's observation and Bernard's theory of hibernation were false.[2] On the other hand, Bernard was not uncritical of the lengths to which Magendie carried his empiricism. He thought that lack of method as an experimental fault might in his day be traced to the influence of Magendie who had been fond of saying: "When I experiment I have only eyes and ears; I have no brain."[3] Bernard said: "This great experimenter was essentially an empiricist. He was even unwilling that a parent thought should direct his experiments; he held that experiments should be allowed to accumulate without preconceived plan and that once amassed they should tell their own story. . . . Magendie was a *victim* of this method or rather lack of method."[4] In the long run, however, Bernard was prepared to forgive Magendie his lack of plan and his antipathy to "reasoning" because his very faults had enabled him to perform a real service for physiology at a time when there was an overwhelming need for exact

[1] xvi, 10. [2] xii, 325. [3] xi, 482. [4] xv, 7.

experimental data.[1] No one was readier than Bernard to do justice to Magendie's penetration and independence, to his honesty and his "absolute respect for facts."

Even before Magendie's death Bernard had begun to take over all the functions of the professorship at the Collège de France, and in 1853–4 he gave the winter lectures on the general subject of blood. These were the first of Bernard's lectures to appear in book form; curiously enough they appeared not in French but in English and were published in the United States. A young American physician from Philadelphia, Walter F. Atlee, attended this course of lectures, and because he wanted his colleagues in America to become acquainted with the advances in experimental medicine in France, asked Bernard's permission to publish the notes which he had taken. Bernard consented and the book appeared under the title of *Notes of M. Bernard's Lectures on the Blood.*

In the following year Bernard determined to publish his own lectures as Magendie had done before him. Thus the famous series of *Leçons* began with the course on "Experimental Physiology" at the Collège de France in the winter of 1854–5. For the next three years the lectures given there were published at the end of the course under the titles, "Toxic Substances" (1857), "The Nervous System" (1858), and "Body Fluids" (1859). After that publication was sporadic. In 1865 a volume devoted to lectures at the Sorbonne appeared, *The Properties of Living Tissues*, to which reference has already been made. From 1864 to the end of Bernard's life his lectures were made immediately available in scientific journals. "Experimental Pathology" appeared by itself in 1871,

[1] ix, 6.

but the lectures had originally been delivered at the Collège de France in 1859–60. "Anæsthesia and Asphyxia," "Animal Heat" and "Diabetes" followed each other in 1875, 1876 and 1877, but none of these later volumes consisted of courses for the current year. They were rather collections of material on the subject indicated gleaned from earlier sources and brought up to date by the inclusion of new experimental material. Bernard's lectures at the Museum of Natural History and his *Operative Physiology* were published posthumously, but he had revised the first volume of the former, the celebrated *Phenomena of Life Common to Plants and Animals*, and the first twenty lectures of the latter.

Even the method of preparing lectures for publication was inherited from Magendie. It was the custom to use notes taken by a student in attendance at lectures and demonstrations as a basis for published work. In Bernard's case the student was usually one who was also working in the laboratory at the time and was therefore familiar with the experimental work in progress. When the student had arranged his notes in what he considered a proper form, they were submitted to Bernard who supervised a final revision. The details of the process were described by M. A. Tripier, who had assisted Bernard in this way from 1854–63, in connection with the posthumous publication of a lecture. M. Tripier says:

> The data which I used for the publication of Claude Bernard's lectures came from several sources. The most important was, up to 1859, making abstracts of notebooks which contained the records of experiments accumulated during nearly twenty years of incessant laboratory work. These transcriptions of experiments were divided into three classes: some were set aside to be supplemented

by future investigations; others were to be used as illustrative material in the teaching then in progress; still others were destined to fill in lacunæ and supply omissions in the lectures already given but not yet published.

In the course of this transcription, there arose theoretical questions, ideas of a new form to be given to certain expositions, and also projects for research. Bernard dictated notes of these to me and they were submitted to the same classification as the notes of experiments and sometimes ended in constituting plans for lectures. . . .

As for the notes taken in the course of lectures, the same method was followed with them as with those dictated in the office: some remained where they were; others were reassigned to previous lectures; still others were put in reserve. This classification was left to me, Bernard reserving to himself the revision of a version that was usually final. . . .

In the lifetime of Bernard, I should, as I had done many times for the publication of lectures which were to be added only later to the edition of his work, have altered and condensed the wording with a view to making it render as best I could the parent idea of the article. Now that Bernard is dead, I no longer felt this liberty permissible.[1]

In spite of the very considerable liberties with the manuscript material that are here admitted to have been taken, the published lectures seem to have retained very closely the quality of the original spoken word. The method of transcription reached its climax when Bernard requested Dr. Benjamin Ball to translate back into French notes of lectures at the Collège de France in 1859–60 which Ball had originally translated into English for publication in the *London Medical Times and Gazette*. Bernard remarked that he wished to have the work done in this way

[1] *Rev. scientif.*, pp. 673–81, 1884.

"in order to render the reproduction as faithful as possible."[1]

The impression one gets from reading the published *Leçons* is that the lecturer was serious, straightforward and enthusiastic, not given to rhetorical flights or picturesque description, but anxious to place before his hearers facts in which he himself was vitally interested. One reason for this impression is doubtless the fact that Bernard drew largely on his own researches for his material. Not only were the different courses built up around the particular branches of physiology which had previously engaged his attention or were at the moment the subject of his research, but before each course was finished, and regardless of the title of the course, he usually managed to touch upon all his major discoveries, viz., the glycogenic function of the liver, artificial diabetes produced by puncturing the floor of the fourth ventricle of the brain, the vasomotor action of the sympathetic nerve and the action of curare. He often began a series by deploring the state of medicine, its lack of a truly scientific basis, and he always ended on the note that much remained to be done. He did consider, however, that the transition from unscientific to scientific medicine had been effected in his time. In 1847 he said that experimental medicine did not exist. In 1869 he recalled this statement and added that after twenty-two years, although experimental medicine had not yet assumed its definitive shape, yet "its dawn was visible on the scientific horizon."[2]

The lectures (especially those at the Collège de France) were well illustrated with demonstrations, and the descriptions of each step in an operation make the reader feel as if he were in the amphitheatre

[1] xi, v.

[2] xi, 468.

and himself an eye-witness. The text shows that at times Bernard met with the not unusual misfortune of the physiologist who attempts a public demonstration and finds his experiment going wrong. On one occasion he administered curare, which normally paralyses all voluntary muscles and causes a quiet death unaccompanied by movement, only to have the animal die in violent convulsions. He was startled, for this had never happened before; and he blamed a new and untested bottle of curare.[1] On another occasion his experiment failed twice in the course of a lecture and at the next meeting he was obliged to confess that his galvanic apparatus had been out of order. When the experiment finally went off perfectly he took advantage of the occasion to bring home to his audience the truth of the principle which he championed throughout his life, that in physiology, as well as in the exact sciences of physics and chemistry, well-conducted experiments always give the same results under the same conditions.[2] There are occasional instances where he tried out a new experiment for the first time before a class,[3] but usually the demonstrations gave evidence of careful preparation.

Sir Michael Foster has criticized the published lectures on the ground that they exhibit "a certain rashness, a lack of carefulness in statement and induction." In his opinion they were undesirable in the hands of a beginner.[4] As a flagrant example he quotes Bernard's statement that stimulation of the vagus nerve causes arrest of the heart either in systole or diastole according to the time of stimulation.[5] This is, of course, an obvious mistake. The heart

[1] Atlee, W. F., *Notes on M. Bernard's Lectures on the Blood*, p. 161.
[2] xi, 217, 219.
[3] v, 147: "It is an experiment which I have never yet done with this special purpose."
[4] *Brit. M. J.*, 1: 519, 1878.
[5] iii, 372.

always stops in diastole, never in systole, when the vagus is stimulated. Bernard himself gives a correct description of the phenomenon elsewhere.[1] Another slip is to be found in his description of dilator nerves. He says:

> The pupil can dilate and constrict; constriction is under the influence of the sympathetic, dilation is attributed to the influence of the common oculomotor nerve. To explain these phenomena the existence of two kinds of muscular fibres which govern these movements has been postulated, radial fibres innervated by the oculomotor which have the function of increasing the opening of the pupil, circular fibres innervated by the sympathetic which upon contraction cause constriction of the pupil.[2]

The true relation of these muscles and nerves is exactly opposite to that stated by Bernard. The radial muscle fibres are innervated by the sympathetic nerve which therefore controls dilation; the circular fibres are innervated by the oculomotor nerve which therefore controls constriction. Perhaps the confusion arose from the fact that cutting these two nerves leads to the opposite effect from stimulating them, as Bernard demonstrated in his course. "If one galvanizes the upper end of a cut sympathetic nerve, all the phenomena which one has seen to result from the removal of the influence of the sympathetic are reversed. The pupil enlarges," and "the pupil which at the *moment* of cutting the oculomotor nerve [i.e., *momentary* stimulation follows cutting] was more constricted than that on the opposite side is to-day the reverse; that is, it is more dilated than that on the opposite side."[3] In the passage where the error occurs Bernard is giving an account of his theory of dilator nerves, viz., that they are in essence paralysing agents

[1] v :393; cf. x, 391. [2] xiii, 232. [3] v, 499 and 205.

and merely annul the action of constrictor nerves; the confusion may have arisen as a result of his thinking of the permanent effect of cutting these nerves rather than of the temporary effect of stimulating them. Still other examples of this sort may be found. Apart from small inaccuracies, a contemporary might have complained that the lectures were more valuable as an exposition of Bernard's own work than as a complete picture of physiology in his time. For instance, he scarcely did justice to Ludwig's work, so closely related to his own, on the chorda tympani. From an historical point of view the range of the lectures is, of course, even more limited than from that of a contemporary. Their chief value consists in the insight which they afford into Bernard's own way of thinking and working.

In the meantime at the laboratory at the Collège de France Bernard's investigations went on. An extraordinary variety of projects was under way during these years. He was continuing experiments on the vasomotor system, and in 1854 he demonstrated before a group of visiting German scientists the change in colour of venous blood in a horse upon cutting and stimulating the sympathetic nerve. Ludwig incorporated in his textbook of physiology of 1856 the results of this striking experiment and Bernard referred in a lecture to his having done so.[1] By 1855 he had established in connection with his researches on carbon monoxide poisoning that this gas replaces oxygen quantitatively in the blood.[2] Simultaneously, he was carrying on experiments with curare and anæsthetics (e.g., chloroform).[3] He revised and enlarged his account of the pancreas,

[1] vi, 267; cf. 426.

[2] Atlee, W. F., *Notes on M. Bernard's Lectures on the Blood,* 19; iii, 157 ff.; vii, 429; xii, 409; *Comp. rend. Acad. d. Sc.*, 47: 393, 1858.

[3] v, 502.

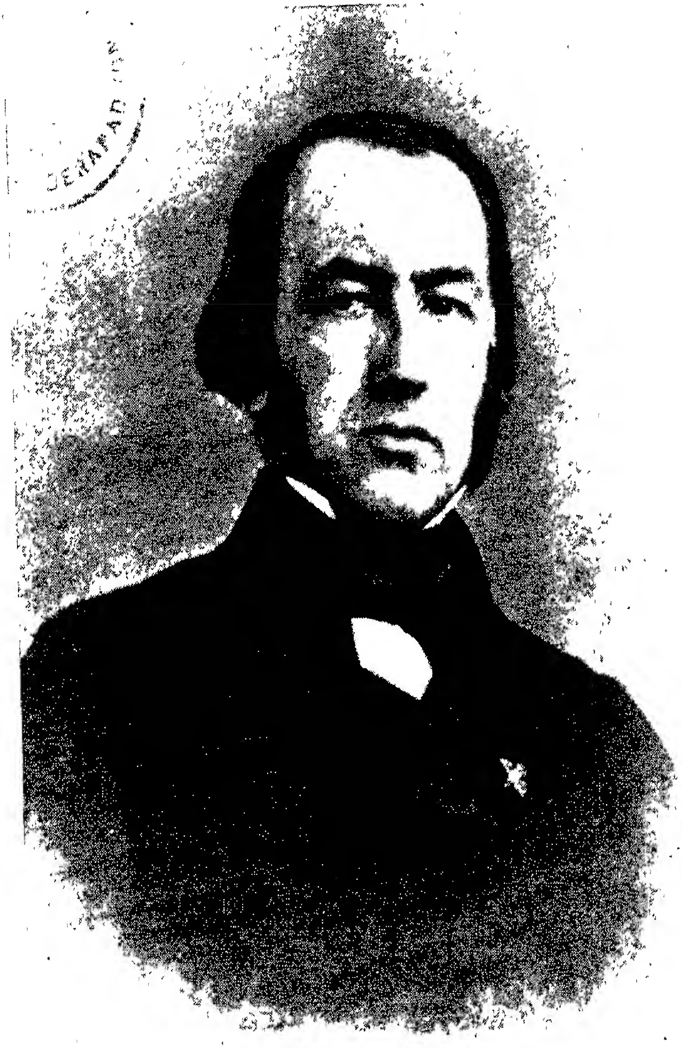
bringing up to date work going as far back as 1846, and the whole was published as a memoir in 1856.[1] He even contributed some papers on the question of spontaneous generation,[2] which, because of Pasteur's spirited attacks, was agitating the scientific world during this decade. Bernard arranged an experiment to show that the growths of mould which readily appear in a solution of gelatine and sugar exposed to ordinary air do not appear if the air reaches the solution after passing through a tube heated red hot. It is claimed that "much of Pasteur's refutation of the spontaneous generation theory was along the lines of Bernard's experiment."[3] This is particularly interesting in view of the fact that Bernard was chairman of the committee of the Academy of Sciences which awarded to Pasteur in 1860 its prize in experimental physiology for work in fermentation. About this time he carried out still another set of experiments on the presence of organisms in the air. These had been prompted by the theory put forward by M. Bréant, the donor of 100,000 francs as a prize to be awarded by a committee of the Academy of Sciences, of which Bernard was the chairman, for the study of the causes and prevention of cholera. M. Bréant, who was not a physician, held that "there existed in the air some organic matter . . . which being introduced into the blood decomposed it and gave rise to cholera."[4] It would appear that Bernard did not arrive at any conclusions which would justify the award of the prize to himself, for the committee continued to meet and the legacy was not distributed in his lifetime. He also acted on a committee appointed by the Minister

[1] *Mémoire sur le pancréas et sur le rôle du suc pancréatique dans les phénomènes digestifs particulièrement dans la digestion des matières grasses neutres*, Paris, 1856.

[2] *Ann. sc. nat.* (*Zool.*), 9: 360–66, 1858; *Comp. rend. Acad. d. Sc.*, 48: 33–4, 1859.

[3] Foster, M., *Claude Bernard*, p. 159.

[4] vi, 487.



CLAUDE BERNARD IN 1849

PLATE III

of Agriculture and Commerce to investigate pneumonia in cattle, and as a result of joint experiments the communicability of the disease was established.[1] Throughout this period he was engaged in experiments on the production of sugar in the liver, and in 1857 he placed the capstone on nearly ten years of work when he isolated glycogen.[2] Furthermore, in the course of tracing the sugar entering and leaving the liver, he hit upon the idea of internal secretion and thus initiated the study of endocrinology.[3] This was the most productive period of Bernard's life. The years between 1846 and 1857 saw the accomplishment of the major part of his lasting contribution to physiology.

Bernard said that "in order to become a physiologist . . . one must live in the laboratory,"[4] and this is virtually what he did. Unfortunately, the laboratory at the Collège de France was a most unwholesome place. Paul Bert described it as "a dark, damp tannery." Bernard did not complain of it on his own account; indeed, he was proud that it had been in Magendie's time the only laboratory in Europe devoted to experimental physiology; but he did regret that the conditions were so bad for the animals. He said:

> It has always been impossible for us to carry on survival experiments or studies in experimental pathology at the laboratory of the Collège de France because the animals had to be kept under such bad hygienic conditions that the phenomena we hoped to observe never developed, or indeed it happened that the animals died of secondary diseases and not of the lesions whose effects we wished to study.[5]

[1] iii, 96. [2] *Comp. rend. Acad. d. Sc.*, 44: 1325, 1857.
[3] *Comp. rend. Acad. d. Sc.*, 40: 589, 1855. [4] xv, 2. [5] xi, 473.

A picture of Bernard at work here in 1859 is given by M. Jousset de Bellesme, who was then a student in chemistry under Berthelot and who later, in 1864, followed Bernard's course at the Collège de France.[1] A casual introduction in a corridor had been the beginning of a friendship which lasted until Bernard's death. M. de Bellesme described the laboratory as a "damp and narrow corridor" and he said that he had gone to see Bernard there and found him standing before his animal table

> . . . with his tall hat on, from beneath which escaped long locks of greying hair; around his neck was a muffler which he scarcely ever took off and his figure seemed a little bent even at his age. His fingers were nonchalantly thrust into the open abdomen of a large dog which was howling mournfully. He turned towards his visitor with a benevolent glance, asking him to wait a moment, and went on with his experiment.

It was so natural to think of Bernard as busy in his laboratory that when statues were later erected to commemorate him both in Paris and in Lyons he was portrayed engaged in animal experimentation. At Lyons he is in a laboratory smock and holds a frog on a board. In Paris he stands with a dog on an operating table beside him, and Paul Bert remarked in his speech at the unveiling of this statue that the sculptor, M. Guillaume, had caught perfectly the expression he had so often seen on the face of his master. "In the midst of an experiment a new fact strikes him. He stops; he reflects an instant. Is this an unimportant incident explicable by the known facts of science? Or is it the result of a condition, unknown as yet, behind which the wisdom of the master sees a discovery?"

[1] xii, 74.

Bernard's energies do not seem to have been completely absorbed even by all this activity in the lecture room and the laboratory. He was assiduous in his attendance at scientific meetings, especially those of the Academy of Sciences and the Société de Biologie, and he was a member of several permanent committees. He also acted upon occasion as a member of special government committees as, for instance, when in 1862 the Emperor appointed him to a consulting committee on Hygiene and Medical Service in the hospitals. That he attached some importance to this appointment is shown by the careful preservation of the document authorizing it among his private papers. To these years also belong some public honours commemorated by decorations which he preserved: the Order of the Polar Star from Sweden and Norway in 1860, and his promotion to the rank of Officer of the Legion of Honour in 1862.

His circle of personal friends included not only the best-known scientists of the day in Paris but also men interested in philosophy and literature. In the ranks of the medical profession his most intimate associates were Dr. Rayer and Dr. Davaine, and he was often to be seen at Dr. Rayer's house. A group whose conversation he particularly enjoyed was in the habit, about 1860, of assembling every week as guests of his friend, Jean Bouley. This group included Berthelot and Renan, his colleagues at the Collège de France; Dr. Péisse, who is reported to have had philosophic leanings; the artist Chenavard; Léon Renault, a rising politician; Deschamps, the poet; Alexandre Weil, the polemicist; and Bernard's own disciple, Armand Moreau. Dr. Genty says that those who met Bernard on these occasions remember him as a delightful conversationalist. He liked the give and take of

conversation among friends, by turns witty and serious, which was characteristic of the end of the nineteenth century.

He also enjoyed talking with men who were meeting with success in the field of literature where his own youthful ambition had received so severe a check. The novel *Germaine*, by Edmond About, the story of a consumptive girl (a favourite character in Continental fiction), was appearing in serial form about 1857. The Empress Eugénie had been so touched by it that she wrote to the author before the concluding instalment begging him not to let the heroine die. Bernard too was captivated by the story and asked About to lunch with him, including in the party his secretary, Dr. Tripier, and the critic Sarcey. The lunch is reported to have been *très gai*, and About was fascinated by his host's conversation which, although on scientific subjects, was expressed in such a way as to seize his imagination as an author. Bernard had been working on those microscopic animalcules, the rotifers, which are to be found in stagnant water everywhere and which have the curious ability to revive after being thoroughly dried (and kept dried for years) if only they are placed in water again. He had already lectured on these queer creatures in his course at the Sorbonne.[1] "Could a man be dried and then, like the rotifer, be revived again?" asked About. Bernard's exact reply is not recorded, but in 1861 About published an amusing novel called *The Man with the Broken Ear*, of which the plot was undoubtedly suggested by the conversation at this lunch. The story deals with one of Napoleon's officers who was by accident nearly frozen on the Russian campaign, and then thoroughly but carefully dried in

[1] x, 46.

vacuum by a German doctor who had been experimenting on rotifers. Nearly fifty years later the Colonel is revived and he expends his first breath in a loyal "*Vive l'Empereur,*" then collapses, gasping, "Where am I? Boy, bring me the Army List." It is said that Paris was convulsed with laughter. A discerning ear can detect parodies, very cleverly reducing sense to nonsense, of Bernard's customary phrases, as for instance in the description of the mummified state of the Colonel: "The globules of the blood would not have been decomposed, but simply agglutinated in the capillary vessels of the dermis or the underlying tissues."

There is no suggestion that Bernard ever played host in his own house at this time. His friends are said to have seen little of Madame Bernard. A little later it became an established custom to have Monday evening gatherings in his laboratory after the weekly meetings of the Academy of Sciences. Here the group was composed mostly of colleagues at the Collège de France and the "disciples."

CHAPTER FIVE

ILLNESS

Quoique souffrant à cette époque, j'acceptai la tâche; je fis de mon mieux. . . .[1]

About 1860, possibly as a result of the unsanitary conditions of the laboratory at the Collège de France where Bernard had during these years been so unceasingly at work, his health began to give way. Although he was able to give his courses as usual, there were no publications from March, 1860, to August, 1862. His chief relaxation every year was a holiday which he took during August and September at his birthplace at Saint-Julien. He was very fond of "smiling Beaujolais" and felt that the country air did him good. Even here he did not entirely abandon his scientific pursuits. He had installed a makeshift laboratory in a shed and there he was in the habit of spending his mornings. He had a few retorts for simple chemical analyses and the neighbouring swamp of Rigodière provided him with frogs. Sometimes, he would collect these "Jobs of the physiologist,"[2] as he called them, on his afternoon walks and bring them home in his pockets. The farmyard also furnished him with material for observation. He was at one time much interested in the career of a hen which dutifully mounted the nest and went through the motions of laying an egg with great regularity. When she left, cackling, it was found either that there was no egg or an egg without a yolk. In reporting the

[1] xvi, 2.

[2] viii, 201.

case history of this bird to Dr. Davaine, his personal physician, Bernard said that the hen finally languished and died, and when he performed an autopsy he found that there was obliteration of the upper extremity of the oviduct and the ventral cavity was filled with yolks which had escaped from the ovary. Dr. Davaine, who was writing a memoir on anomalies in eggs, passed on this communication from his friend and patient to the Société de Biologie in 1860.[1]

Later in the day Bernard would walk about his property, sometimes carrying a small spade which he used to transplant bushes, sometimes grafting trees. He liked to go about the country-side talking the local patois with neighbouring vine-growers, and he actively superintended the making of the wine from the family vineyards at vintage time in September. He was proud of the product of these vines and friends who came to see him were apt to be invited to taste samples and pronounce upon them. He occasionally had visitors who came down from Paris and he accepted invitations to dine with the Comte de Tournou, the owner of the local *château*.

In 1861 he purchased for 60,000 francs a house on the hill of Châtenay above Saint-Julien, which had originally been the manor of the estate of which his birthplace had been merely the farm-house, and, along with it, a vineyard extending down the slope in front. The buildings are arranged around a courtyard which the front of the farm-house overlooks. Bernard's newly acquired house has its back to the sheds on the far side of this court and itself faces the vineyards in the direction of Villefranche. The village of Saint-Julien is tucked away at the foot of the steep hill

[1] *Mém. Soc. de biol.*, 12: 182–263, 1860.

behind the farm-house. The latter is a crude two-story structure with irregularly placed windows and doors. The house which Bernard was in future to use for himself is a long three-story building covered with tawny yellow stucco, its regularly spaced windows and doors fitted with heavy shutters. There are still the remains of a lawn in front of it and to the right among trees is the level spot where Bernard laid out a bowling-green.

Meanwhile, in spite of interruptions arising from illness and country holidays, his professional life went on. It was not until 1861 that he was elected to the Académie de Médecine, although he had long been worthy of the honour. The reason for the delay was that his entrance would naturally have come through the section of physiology. He had applied in 1845 on the death of W. Edwards, but his name did not appear in the list of nominees and M. Longet was elected. Until the death of M. Duméril in 1861 there was no further vacancy in this section. By this time Bernard's reputation was so great that his election was accomplished by a nearly unanimous vote (72 out of 79). He had always been aware of a certain patronage in the attitude of the clinician towards the laboratory worker and something of this patronage is, I think, to be found in the comment made by M. Jules Guérin in the *Gazette médicale* on Bernard's election:

> M. Claude Bernard is no ordinary worker; he is now the principal representative of experimental physiology. He not only employs the experimental scalpel with all Magendie's skill, but he uses chemical analysis with the same success, being both a consummate anatomist and an accomplished chemist. On all these accounts his entrance into the Academy has the significance of an idea, of a

> principle, which doubtless explains the unanimity of the reception which he has experienced. If M. Bernard so understands it, he will not fail in his duty, but will regard his position seriously and enable the Academy to profit by the eminently scientific character which he has been able to confer upon his investigations.[1]

Bernard was never perfectly at home in the Academy of Medicine. He disliked the pomposity which seemed to him characteristic of members of the medical profession, as is shown by a remark of his to Pasteur, who was not a physician, when the latter joined him at the Academy in 1873. "Have you noticed," he asked, "that when a doctor enters a room he always looks as if he were going to say, 'I have just saved the life of a fellow man'?"[2] This dislike was very probably rooted in recollections of the unsympathetic and reactionary attitude of at least some physicians to experimental physiology at the beginning of his own career. Although he realized that the more forward-looking members of the profession shared his views with regard to the place of physiology in medicine, nevertheless he stated in his first published lecture that there were still physicians who regarded physiology as of no practical use in medicine but "a science de luxe" which could perfectly well be dispensed with by medical students.[3] Even towards the end of his career, we find Dr. E. Callamand, who attended his lectures from 1875 through 1877, remarking that there were in an audience of fifty or sixty "few or no medical students: they were in the hospital, and were concerned more with the clinic and preparation for their examinations than with experimental medicine." There was

[1] *Gaz. méd.*, 16: 151, March 16, 1861.
[2] Vallery-Radot, R., *La vie de Pasteur*, p. 301. [3] i, 29.

also objection in the later years of Bernard's professorship to his use of the chair in Medicine at the Collège de France for what was in reality an exposition of the science of physiology, and the suggestion was even made that the word "medicine" be dropped from its announcement.[1] So acutely did he feel these "ill-founded reproaches" that he devoted some part of the opening lecture of nearly every course he gave at the Collège de France to showing how his method of teaching ought to differ from that in the Faculty of Medicine.

Remarks dropped here and there in the lectures show that Bernard was very conscious of the long-standing points of disagreement between the pure scientist and the clinician. He accuses physicians who have devoted themselves to clinical observation for a long time of refusing to admit that physiology is the basis of medicine and of distrusting all other methods of medical investigation except their own.[2] He says that "the progress or modification of physiological theories exercises for the most part only an uncertain influence on the art of healing and we even see a great number of practitioners isolate themselves completely from physiology as if it were useless and even dangerous to follow its various fluctuations."[3] He resents the accusations made by the physician that the physiologist lacks the proper bent to make a good practitioner.[4] He brushes aside the physician's claim to be regarded as an artist,[5] and makes the countercharge that the average physician of his day was an empiricist; indeed he says that that is all a physician *could* be until with the advance of medicine he was *able* to become a scientist.[6] He combats the (in his

[1] xi, 461, 462; xiv, 3. [2] xi, 316. [3] xi, 3–4.
[4] xi, 322. [5] viii, 356. [6] viii, 357.

day) stubbornly persistent idea that a distinction should be made between physiological and pathological conditions. Two physicians, the celebrated Trousseau and Dr. Pidoux, as late as 1855 had said in their *Treatise on Therapeutics* that "It is not within the power of physiology to explain the simplest pathological affection." Bernard meets this obscurantism with sweet reason. He says:

> Medicine is the science of sickness; physiology is the science of life. The latter is therefore more general than the former. That is why physiology should be the scientific basis of medicine. The physician is one who studies the sick man and uses physiology to enlighten and advance the science of disease. The physiologist is one who studies the science of life and seeks to bring into line with it the science of sickness. He takes the point of view of pure theory and is not interested in immediate practical values. The physician, on the contrary, is occupied above all in application, and he seeks to render this application scientific by leaning on the findings of physiology.[1]

Bernard seems to have resumed experimental work some time in 1862 for in the early autumn he published two articles on the sympathetic nerve.[2] At about this time one of Ludwig's favourite pupils, Dr. Sechenov, who was destined to become Pavlov's teacher and the father of Russian physiology, came to work in Bernard's laboratory. He brought his own problem, the inhibitory action of the brain, and apparently worked independently. When his work was published it was dedicated to Ludwig with no mention of Bernard. It is possible that Bernard was not in very close attendance at the laboratory at this time, which may explain Sechenov's apparent discourtesy. However,

[1] xiv, 52.
[2] *Comp. rend. Acad. d. Sc.*, 55: 228–36, 305–12, 341–50, 381–8, 1862.

Bernard gave a full report of Sechenov's work in one of his lectures two years later.[1]

Any physiologist discussing the subject of inhibition always begins with the discovery of the inhibitory action of the vagus nerve on the frog's heart, a discovery universally attributed to the Weber brothers who made the observation in 1845 but did not publish it until the following year. Now Bernard had independently observed the same effect in 1846. He had listened to the heart sounds in the dog while stimulating the vagus nerve and had found that the sounds ceased but were heard again when he stopped stimulating the nerve. This fact was recorded not in a paper by himself but in a thesis by one of his pupils, Dr. Lefèvre, which was not published until 1848. Bernard always insisted that because of his observation France had as good a claim to this discovery as Germany.[2] Sir Michael Foster's statement that "Bernard never grasped the real bearing of the result which he had observed"[3] is hardly borne out by Bernard's discussion of Sechenov's work, where the whole subject of inhibition is quite adequately treated.

Bernard for some years had carefully followed Pasteur's work on spontaneous generation and it will be remembered that he had tried his own hand at some experiments along these lines as early as 1858. In the spring of 1863 he actually collaborated with Pasteur in experiments on the putrefaction of dog's blood and urine. The part played by Bernard is disclosed in a footnote to Pasteur's paper which states, "I will add, in order that you may be assured that the experiments were well conducted, that M. Claude Bernard was so obliging as to preside himself over the

[1] x, 356. [2] ix, 66, 195. [3] Foster, M., *Claude Bernard*, p. 138.

taking of blood."[1] The samples were placed in sealed vessels and kept at 30° C. from the 3rd of March to the 20th of April. Pasteur was able to produce these vessels before the Academy and show that no putrefaction had taken place.

Although there is this evidence that Bernard was concerned in Pasteur's experiment in 1863, nevertheless his own publications were for a second time interrupted in the autumn of 1863 and not again resumed until the summer of 1864. This time his winter course for 1863–4 at the Collège de France was omitted and Paul Bert, his favourite pupil, states that his illness had become so severe that he was compelled to leave the laboratory and Paris for the country.[2] However, the reason which he himself gave for the omission of his winter course when he resumed his lectures the next spring was that the administration had at last consented to make improvements in his laboratory which consequently had not been available for the purposes of the course.

It may be significant that the subject of his research on his return to work was opium and its derivatives[3] in view of the fact that his disease was characterized by severe abdominal pains which were not continuous but recurrent. It is not implied that in this case Bernard served as the experimental animal, for he remarks that he found frogs more uniform in their reaction to morphine than dogs, but less sensitive, so that he used young sparrows "which were very abundant in Paris that spring."

His first glimpse of what he described in a letter to a friend as "social and political splendours" came

[1] *Comp. rend. Acad. d. Sc.*, 56: 734, 1863; cf. Vallery-Radot, p. 116.
[2] xviii, 24; cf. xi, 415.
[3] *Comp. rend. Acad. d. Sc.*, 59: 406–15, 1864.

with an invitation from the Imperial court in 1864 for one of the famous "six days" at Compiègne.[1] On each of these occasions the Empress Eugénie carefully superintended the list of guests, choosing politicians, authors and scholars from a long list of celebrated people prepared for her by the court chamberlain. The guests arrived from Paris on a special train accompanied by their wives and mountains of luggage —since crinolines were in fashion—and they were driven in the Imperial carriages from the station to the *château*. The mornings were left free and in the evening the formal dinner was followed by amateur theatricals or the declamations of poets or even experimental demonstrations by scientists, for example, Pasteur or Longet. The early afternoons were spent out-of-doors in walks, drives or hunting expeditions, but at tea-time the Empress liked to have her guests gather around her table, and she expected the scholars and the wits to converse on topics which she suggested. The Emperor preferred to take a single guest aside and talk to him alone, and Bernard was honoured in this way. The Emperor said that he regretted that he knew nothing of the science which he had been told that Bernard represented and he asked for information about it. Bernard is said to have talked physiology steadily for two hours, much to the amazement of the court. The Emperor, however, was fascinated and said finally, "You are a great man of science and I want you to be pleased with me." He called M. Duruy, the Minister of Public Instruction, whom Bernard had known when he was a rather undistinguished professor, and gave orders in the best tradition of the fairy tale that Bernard should have whatever he wanted. When

[1] *Raff. Corr.*, 3654, 3.

the Minister called upon Bernard a few days later to learn his wishes, Bernard's only thought was for the needs of his science, namely, well-equipped physiological laboratories. M. Duruy explained that what the Emperor had had in mind was something more personal, something for Bernard himself. Thus pressed, Bernard admitted that he would like a laboratory assistant, and it was shortly after this that Paul Bert became his assistant at the Sorbonne.

Bernard seems to have used the leisure which was forced upon him by his first attack of illness between 1860 and 1862, and by its recurrence during 1863 and 1864, to carry out a plan which, he says, had first occurred to him as early as 1858.[1] He was dissatisfied with the discursive and fragmentary nature of his published lectures and papers and he conceived the idea of writing a comprehensive treatise on experimental medicine. His inability to carry on work in his laboratory during his illness left him free to collect and shape his thoughts for, as he said himself, when his malady attacked him he could do little more than think. When he had been compelled to withdraw to Saint-Julien, he would sit on what he called the *banc de Sisyphe*—a mild professorial pun, since the bench was placed in front of six yew trees (*six ifs*, in French)—and look out over the vine-clad hills towards Mont-Blanc while he pondered over his plans for the huge work which would gather together into a co-ordinated whole all of what he termed the "scattered fragments of my studies."[2] In the intervals of pain he tried to probe deep into his mind to see just how it was that he had happened to come upon the discoveries he had made, what were the mental processes which had led him to adopt one experimental method rather

[1] xi, v. [2] xi, vi.

than another and what were the general principles underlying the existence of living creatures.

M. Tripier, who, as has been stated, acted as Bernard's secretary between 1854 and 1863, intimates that a first draft of the introduction to the contemplated treatise was completed in 1862, but it was only in 1865 that Bernard accomplished the final revisions and decided to publish by itself this introductory part of his projected work under the title of *Introduction to the Study of Experimental Medicine.* As it happened the main body of the work was never written and the *Introduction* stands alone as a disquisition on method.

It is probable that the preparation of the *Introduction* was Bernard's first real attempt at composition since his youthful effort in the field of romantic drama, for the published lectures were really the transcription of his extemporary speech and his scientific papers were mere formal records of experimental facts and conclusions. It is true that M. Tripier's expressions in a letter to M. d'Arsonval in 1906 might be construed to mean that he had exercised his revisory powers even in this case,[1] but at least the point of departure was a manuscript written in Bernard's own hand, which is still in existence and in the possession of his last assistant and eventual successor to the chair of Medicine at the Collège de France, M. d'Arsonval. This document consists of a green MS. book of the sort often used for accounts, labelled on the outside cover with a gummed postage stamp margin bearing the single written word *Livre.* The precise nature of the contents can be

[1] "Having come to him [Bernard] as secretary for the publication of his lectures in 1854, I was in his confidence until 1862, after the publication of the lectures on the nervous system and a first preparation of the *Introduction to Experimental Medicine*": letter from A. Tripier to d'Arsonval, September 9, 1906, quoted in *Méd. gén. fr.*, 2: 12, 1935.

judged only from isolated pages which have been photographed and reproduced in various journals,[1] for M. d'Arsonval, who intends to bequeath the manuscript to the museum of Bernard's relics which will in time be installed in his old laboratory at the Collège de France, has not permitted it to be closely examined by the present writer. It has been described as a succession of entries dealing with Bernard's work and opinions, made from day to day, and dating from 1862 to 1878. The passages selected for reproduction have intentionally been those not included in the version which was published in 1865 and they are therefore not very useful in establishing the exact relationship of the manuscript to the printed book. The essential ideas of the available passages are those with which we are already familiar in the *Introduction*. It is difficult to judge the language from such brief extracts, but one suspects that the style is a shade more abrupt and colloquial. For example:

> However good a theory may be, it is never as good as the truth or as actual fact. I do not believe that there is a single theory in physiology, or even in physics and chemistry, which is actually and absolutely true. Everything is merely relative. It is when we reach the ultimate that we shall have the absolute. It is therefore an excellent thing to have destroyed a theory. It is a step forward, and we must not quake when a fact destroys a theory, even our own—we must investigate. There is a discovery underneath, a revolution, as they say (for science is revolutionary) [*sic*] and does not advance by successive additions as people believe it does.

The relation of fact to theory is the subject of a whole section of the second chapter of the first part

[1] Cadilhac, P.-E., *L'Illustration*, p. 80, January 19, 1935. Godlewski, H., *Méd. gén. fr.*, 1: 424, 1934.

of the published *Introduction*, but nowhere does the phraseology duplicate that of the passage quoted. The idea is one which may be found in Bernard's earliest lectures—"theories, however artistically they may be constructed, can never have the value of a well-established fact"[1]—and it is restated as one of the principles of experimentation towards the conclusion of the *Introduction*: "When a fact which one meets is in opposition to a reigning theory, one must accept the fact and abandon the theory, even though it is universally held through the influence of great names."[2] Another of the passages reproduced has its closest counterpart in the first of the lectures on the "Phenomena of Life Common to Plants and Animals" which belong to the end of Bernard's life and there is nothing very like it in the *Introduction* itself.[3] It is tantalizing that no passages have been reproduced which are verbal counterparts of passages in the published *Introduction* since the d'Arsonval document has been repeatedly referred to as the actual manuscript of the *Introduction to Experimental Medicine*. One would like to see in Bernard's own handwriting a few sentences which have been transferred unaltered to the printed book, and so be assured that the manuscript *Livre* is not merely a repository for reflections which Bernard regarded as suitable raw material for the finished work.

It was probably also at about this time that Bernard prepared the popular article on curare which was published in the *Revue des deux mondes* in September, 1864. The material for this article is all to be found in his lectures at the Collège de France for 1856;[4]

[1] i, 292. [2] viii, 288.
[3] d'Arsonval, A., *Méd. gén. fr.*, 2: 13, 1935; cf. xvi, 43–4.
[4] *Rev. d. deux mondes*, pp. 185–90, September 1, 1864; cf. iii, 238–348; xviii, 237–315.

but he succeeded in giving it a more literary dress, and certain passages describing the visible effects of curare poisoning on men and dogs attain a marked vividness of expression.

A little over a year later he published a second popular article in the *Revue des deux mondes* entitled "Of the Progress of the Physiological Sciences."[1] In this article he discussed the relation of scientific determinism to experimentation on living things and referred to his own particular doctrine of the "internal environment" which he had been preaching in his lectures for nearly ten years. The essay as a whole is a résumé of the ideas treated at length in the *Introduction to the Study of Experimental Medicine*, and it is embellished by brief quotations from Goethe, Molière and Pascal. We know that Bernard had at Saint-Julien a copy of Pascal's *Pensées* which he was in the habit of taking to bed with him to read himself to sleep.[2]

One of the periodic epidemics of cholera occurred in 1865 and at its peak in October it exacted a toll of two hundred victims a day in Paris. Bernard for some ten years had been interested in this disease through his chairmanship of the committee of the Academy of Sciences for the distribution of M. Bréant's legacy.[3] Pasteur, turning aside from his work on the silkworm disease, now joined Bernard and Saint-Claire Deville in a study of cholera. They went to the attic of the Lariboisière hospital above a cholera ward. There they made an opening in the ventilator from the ward below and attached to it a glass tube surrounded by a cooling mixture. In this way they hoped to collect the germs by concentrating

[1] *Rev. d. deux mondes*, pp. 640–63, August, 1865; cf. xviii, 37–98.
[2] *Raff. Corr.*, 3653, 43.
[3] *V. infra*, p. 84.

the air. Bernard drew blood from cholera patients and took samples of dust from the ward. In none of these experiments did the collaborators meet with any success.

Ordinarily Bernard's travels were nothing more than a shuttle-like movement back and forth between Paris and Saint-Julien. In August of 1865, however, occurred one of his rare excursions farther afield. He went to the south of France, near Perpignan, as one of the deputation to represent the Academy of Sciences at the inauguration of a statue to the astronomer, François Arago, in the village of Estagel. It was as he stepped off the train that young Georges Barral for the first time saw Bernard and the vision prompted the following outburst:

> I had just got down from the coach . . . when I saw the tall form of a person whom I did not yet know majestically poised upon the steps. It was like a palpable vision. I thought that I saw one of the great modern saints in lay dress, a sort of Vincent de Paul of science. It was very hot, and the setting sun, the ruby sun of the Pyrenees, shone out in all its flaming splendour. His head with its sculptural lines, was uncovered, and there was a nimbus sketched in by the evening light. My adolescent imagination was so struck by the severe and majestic beauty of the celebrated scientist, who was then in the full physical and mental force of his age, being scarcely fifty-two, that I have never forgotten the spectacle. I close my eyes and still see it as if it were photographed on my retina.[1]

During the several days of celebration, Bernard was a fellow guest with Georges Barral and his father of the deputy of the department, and this was the beginning of a long and close friendship between Bernard and the Barrals. In October, after their

[1] Barral, G., *Préface historique* to *Arthur de Bretagne.*

return to Paris, both Bernard and the elder Barral were stricken with illness, and Georges Barral states that both had a light attack of cholera. In Bernard's case this is vigorously denied by subsequent biographers.[1] He was probably suffering from an especially severe recurrence of his long-standing abdominal trouble, which was never satisfactorily diagnosed. Sir Michael Foster has suggested that nowadays the disease would have been recognized as appendicitis, but this is very doubtful.[2] M. Jousset de Bellesme, who was a pupil of Bernard's and a physician, says that it was chronic enteritis with symptoms affecting the pancreas and liver.

Dr. Rayer, the Emperor's own physician, and Dr. Davaine were in attendance on him in 1865, but they were unable to make any headway in combating this attack and gave up hope of his recovery. He was unable to give his winter course at the Collège de France and on the 22nd of April, 1866, he left Paris for Saint-Julien accompanied by Dr. Davaine, his wife and daughters remaining in Paris. Bernard in reality had little faith in medicine as practised in his day, and he preferred to rely, as Pasteur said, upon nature itself.[3] At Saint-Julien he lived simply and carefully, and made a daily study of his symptoms to guide him in his regimen. Beyond the bowling-green in front of his house were the six small yew trees and in their shade the *banc de Sisyphe*. Here he liked to sit when the tedium of his disease prevented greater activity. He was only fifty-three years old and full of plans for future work. Experimental investigation had been his whole life, and now to be entirely cut off from the scientific world left

[1] Genty, M., *Biogr. méd.*, p. 145, 1932.
[2] Foster, M., *Claude Bernard*, p. 181.
[3] *Œuvres de Pasteur, réunies par Pasteur Vallery-Radot*, ii, 486.

him a prey to such profound melancholy that his friends were alarmed. Many of them sent him cheering letters. A telegram from the Emperor asking for news of the illustrious invalid created a buzz of excitement in the village. Pasteur conceived the idea of publishing an article in praise of Bernard, and this appeared on November 7, 1866, in *Le Moniteur universel*, the official journal of the Second Empire. In this article Pasteur showed how science had been enriched by Bernard's discoveries, taking as an example his work on the glycogenic function of the liver, and he prophesied that the science of the future would be greatly influenced by Bernard's methods which were so clearly and beautifully disclosed in his recently published *Introduction to the Study of Experimental Medicine.*

When Bernard read this article, he at once wrote the following letter to Pasteur:

> My dear Friend,
>
> Yesterday I received the *Moniteur* containing the superb article you have written about me. Your great praise makes me very proud, although I feel I am yet very far from the goal I would reach. If I am restored to health, as I now hope I may be, I think that I shall find it possible to pursue my work more methodically and with more complete means of demonstration, better indicating the general idea towards which my various efforts converge. In the meantime, it is a very precious encouragement to me to be approved and praised by a man such as you. Your work has given you a great name, and has placed you in the first rank among experimenters of our time. The admiration which you profess for me is indeed reciprocated; and we must have been born to understand each other, for true science inspires us both with the same passion and the same sentiments.
>
> Forgive me for not having answered your first letter:

but I was really not equal to writing the notice you wanted. I feel deeply for you in your family sorrow. I too have passed through this trial and I can understand how some-one as sensitive and tender-hearted as yourself must have suffered.

I intend to return to Paris soon and give as much of my course this winter as I can. As you say in your article, the acute symptoms seem to have disappeared, but I still need to be very careful; the least fatigue, the least departure from my regimen, puts me on my back again. Besides, I have received in the course of my illness so many indications of sympathy and of great goodwill, so many proofs of esteem and of friendship, that it seems to me that I am bound to neglect nothing that may re-establish my health in order that I may eventually be able to express my thanks and devotion to some, and to others my sincere affection.

I hope that I may see you soon; in the meantime I am your devoted and affectionate confrère,

CLAUDE BERNARD

Saint-Claire Deville at the same time surprised Bernard with a letter to which were appended the signatures of a number of his friends. In response, he wrote to Saint-Claire Deville the day after he wrote to Pasteur as follows:

MY DEAR FRIEND,

You are as clever at inventing friendly surprises as at making great scientific discoveries. It was a charming idea and one for which I am very grateful to you, that of sending a collective letter from my friends. I am carefully preserving this letter, partly because it expresses sentiments which I value very much, and partly because it is a collection of the autographs of illustrious men whose names will go down to posterity. I beg you to convey my thanks to our friends and colleagues, E. Renan, A. Maury,

F. Ravaisson and Bellaguet. Tell them how deeply grateful I am to have them remember me and congratulate me on my recovery. It is unfortunately not yet a cure, but at least I hope that I am on a fair way to it.

I received the article which Pasteur wrote about me in the *Moniteur*. This article paralysed the vasomotor nerves of my sympathetic system and caused me to blush up to my eyes. I was so amazed that I do not know what I wrote to Pasteur; but I did not have the effrontery to say to him that he had perhaps been wrong to exaggerate my merits. I know that he believes all that he writes, and I am happy and proud of his opinion, because it is that of a first-rate scientist and an unsurpassed experimenter. Nevertheless, I cannot help thinking that he has seen me through the prism of feelings dictated by his kindly heart, and I do not deserve such extravagant praise. I am more than happy over all the marks of esteem and friendship which have come my way. They make me cling to life and show me that I should be very stupid not to take care of myself in order that I may go on living among those who feel affection for me and for whom I have a like feeling because of all the happiness they give me. I intend to return to Paris at the end of the month, and, in spite of your kind advice, I should like to resume my course at the Collège de France this winter without being too strenuous about it. I hope to be allowed to postpone its opening until January. But we shall talk of all this in Paris. I remain your devoted and affectionate friend,

CLAUDE BERNARD

Saint-Julien,
Saturday, November 10, 1866

Both these letters show how deeply Bernard was touched by Pasteur's tribute. As so often happens, he wrote best when he was stirred. A week later, he wrote again to Pasteur, conveying his thanks more formally:

My dear Friend,

I have received compliments on all sides with regard to your excellent article in the *Moniteur*. I am very happy and it is you to whom I owe my thanks, since you have made me an illustrious man through your scientific prestige. I am in haste to resume my work and to see you again, as well as all my friends in the Academy; but I could wish that my health were a little better established. It is fine weather here; that is why I am delaying my return to Paris for a few days.

Your very devoted and affectionate colleague,

Claude Bernard

He was to be disappointed in his expectation of returning to Paris so soon. He was too optimistic and it was not until the middle of July, 1867, that he could leave Saint-Julien. Even then he did not resume his lectures or his laboratory work, but he was able to go on with his writing.

During 1867 two of his very close friends died, M. Pelouze and Dr. Rayer. Dr. Rayer had been president of the Société de Biologie of which Bernard had been a most active member since its founding twenty years before, and, as has been mentioned, Bernard was now elected to the perpetual presidency of this society. Among other honours which came to him at about this time were his foreign membership in the Royal Society of London[1] (1864) and his honorary memberships in the Academy of Sciences of Berlin and of St. Petersburg; in fact, in all the important scientific societies between Stockholm and Constantinople. At home he was advanced to the rank of Commander in the Legion of Honour in 1867.

[1] On November 30, 1876, he was awarded the Copley Medal of the Royal Society, which was received on his behalf by the French Ambassador.

CHAPTER SIX

PUBLIC RECOGNITION AND PRIVATE DISAPPOINTMENT

Les ennuis et les honneurs qui m'ont accablé à la fois. . . .[1]

WHILE BERNARD was still away from Paris, Napoleon III grew eager to impress the world with the glories of France and, following the lead of Victoria's consort, Prince Albert, he decided upon a great exhibition as the best means of accomplishing his end. It was to be shown that France was superior to the rest of the world not only in a material way but in the realm of intellect as well. The Minister of Public Instruction, M. Duruy, was to invite the appropriate scholars to make reports on the progress of letters and sciences in France and these reports were to be published by the Imperial Press at the public expense. Bernard was chosen as the representative of physiology and he wrote his *Report on the Progress and Achievements of General Physiology in France* in which he attempted to show the part which Frenchmen had played in the development of this science.

It was natural that he should claim for his own master, Magendie, "the glory of having definitely planted the flag of physiological experimentation."[2] He did not hesitate to record in detail the struggle over priority for the discovery of the functions of the dorsal and ventral roots of the spinal nerves between Magendie and Sir Charles Bell, and to charge that the friends of the latter had issued as a reprint bearing

[1] *Raff. Corr.*, 1869. [2] ix, 7.

the original date a modified version of Bell's first article in order to make it appear that all the credit should go to the Scotsman. Bernard, on the contrary, claimed this credit for Magendie. This discovery, he said, "belongs to France."[1]

For discoveries which were unequivocally French he had to rely almost entirely upon his own, although he referred to a number of minor discoveries, especially those made by his friends, Rayer, Davaine, Paul Bert and Brown-Séquard, and to Pasteur's important work on spontaneous generation. It is curious that he seems to have missed the significance of Brown-Séquard's work on the adrenal glands in 1856–8, choosing to mention his very much less important work on transfusion. Again, the restriction of his survey of French physiology to the preceding twenty-five years did not prevent full discussion of work done by Magendie earlier than that, while it evidently justified in his mind the omission of Flourens's classic experiments on the removal of the cerebrum and cerebellum in pigeons. It must be remembered that Flourens had taken the wrong side in the Bell–Magendie controversy.

The concluding paragraphs of the "Report" give the gist of his argument:

> We have seen that French physiology has maintained its lead through the initiation of ideas and discoveries. Its achievements have been numerous and important and it is astonishing that so much has been accomplished with so little means. . . . To advance in physiology as in other experimental sciences, two things are necessary: genius, which it is not in our power to bestow; the means for work, which can be provided. French physiology demands only that which it is easy to give; genius has never been lacking.[2]

[1] ix, 157.

[2] ix, 148–9.

In short, the *Report* is a plea for better physiological laboratories in France. This is also a subsidiary theme in an article concerned chiefly with the principle of determinism in the biological sciences which appeared in the *Revue des deux mondes* simultaneously with the publication of the *Report* in December, 1867.

Bernard's object was not a selfish one for he felt that if any changes were to be made the benefits should not be exclusively enjoyed by a single physiologist or even by a select few.[1] What was in his mind was the provision of laboratories where the younger physiologists could be initiated into the art of experimentation or carry out their own experimental projects. He remembered the career of his friend, Brown-Séquard, who had been so closely associated with him in the founding of the Société de Biologie twenty years before. Brown-Séquard after taking his medical degree had remained in Paris, housing both himself and his experimental animals in a wretched unheated garret where the odours were overpowering. Bernard thought it a pity that France could find no place for a young man who would persist in valuable experimental work under such discouraging conditions, and that for want of support Brown-Séquard, six years after he had taken his medical degree, should have been forced to leave France and seek a livelihood across the Atlantic, where, after many vicissitudes, he had been established since 1863 as professor of the Pathology of the Nervous System at Harvard. In 1867 he returned to Paris to accept for a brief time the chair of Experimental Pathology in the Faculty of Medicine. Bernard was among the first to welcome and congratulate him on his return

[1] xi, 473.

to France and made a point of attending his opening lecture. It makes a fitting end to the story of Bernard's solicitous interest in Brown-Séquard that his own laboratory, the one which he had inherited from Magendie, should in turn have passed to Brown-Séquard who succeeded him in the chair of Medicine at the Collège de France.

Before Bernard's protest against the state of the laboratories had been published, Pasteur, in his more impetuous manner, had made his own protest in the form of a letter addressed directly to the Emperor.[1] He asked for an adequate laboratory with facilities for experiments as dangerous to health as those on putrefaction, gangrene and the viruses were likely to be. He pointed out how valuable his researches had been to agriculture and medicine. The Emperor was impressed and set in motion the governmental machinery for securing the necessary credit for the improvement of Pasteur's laboratory. Plans were drawn for an addition to the École Normale, but nothing more happened. At the end of the year Pasteur learned that the credit had been stopped and the laboratory was not to be built, although work on the Opera House was to go on. Millions of francs were available to decorate that ornate structure and build the curving ramps up its sides so that Napoleon and Eugénie could be driven behind prancing horses direct to the door of their box, but not a franc was forthcoming for the new laboratory. Pasteur saw red. He sat down at once and wrote an impassioned protest. He said:

> A few days ago two members of the Academy of Sciences were speaking of one of our foremost chemists, at present in bed with pneumonia. "What can you expect?" said

[1] Vallery-Radot, R., *La vie de Pasteur*, p. 191.

> one. "The laboratories are the tombs of scientists." It was Claude Bernard who spoke, the physiologist whom Europe envies us and who through a miracle has just recovered from a long illness, the origins of which in his case also were to be sought in his laboratory. . . . What is this establishment where the laboratories are so unhealthy, humid, dark and poorly ventilated? . . . It is the Collège de France.[1]

This article was intended for the January, 1868, number of the *Moniteur universel*, the official government bulletin; but when it came into the hands of the editor he fairly leaped from his chair, for it was in fact a direct attack upon the administration. The *Moniteur* refused to publish the article, but it appeared in the *Revue des cours scientifiques* for February, with the result that Napoleon called a conference on March 16 at which Pasteur, Bernard, Saint-Claire Deville and Milne-Edwards represented science and Duruy, Rouher and the Marshal Vaillant represented the administration. The general question of the need for research laboratories in France was discussed, and the suggestion which was finally acted upon in regard to Bernard came from Pasteur who had pointed out the possibility of finding the space at the Museum of Natural History which was not available at the Collège de France. A decree was approved in August, 1868, regarding the laboratories for teaching and research. Work on Pasteur's laboratory at the École Normale was begun at once. As a result of the provisions of the decree dealing with physiology Bernard immediately gave up to his pupil, Paul Bert, the chair which had been created for him at the Sorbonne and transferred his course in General Physiology to the Museum of Natural History.

[1] Pasteur, L., *Rev. d. cours scient.*, p. 137, February 1, 1868.

He was full of interest in the new arrangements during 1869 and his references at this time to the laboratories of W. Kuhne, his former pupil, at Amsterdam and of Ludwig at Leipzig, which were also new, have a distinctly envious ring. He even went into detail about the appointments at the latter establishment, which included a suite for the professor to live in, and he said that he had been influenced by the arrangement of Ludwig's laboratory in planning his own.

> A physiological laboratory needs to combine these three points of view, these three indispensable bases of the science of life: animal experimentation, physiological chemistry and histology. . . . The laboratory of Ludwig is therefore perfectly organized since it satisfies this threefold demand: and so it is the same point of view and an analogous plan that I shall endeavour to develop in the physiological institute which has been promised me in connection with my chair in General Physiology at the Museum of Natural History.[1]

His new laboratory, as a matter of fact, was not ready for occupation before the summer of 1870.

Bernard was now a public figure. He complained in a letter written in 1870 that since he had received his first invitation to Compiègne six years before he had seen enough of the social and political world to disillusion him and make him eager to return to his laboratory which, he felt, was his real element.[2] Although since the time of the complete breakdown in his health he had made a rule against accepting invitations to dinner because he was obliged to eat

[1] xi, 540–3. Although Bernard never succeeded in including a histological section in his own laboratory, he did persuade the government to establish a histological laboratory at the Collège de France and to appoint in 1875 his pupil, L. Ranvier, to the newly created chair (xiv, 48).

[2] *Raff. Corr.*, 3654, 3.

sparingly or not at all in the evening, nevertheless he made certain exceptions, and we find the Goncourts referring to him in their celebrated *Journal* as a guest at the Princesse Mathilde's in April, 1868, and again in December.[1] On the former occasion he is described as one of two "ghosts" among the diners. The other was the poet, Gauthier, "pale, his leonine eyes sunken." Bernard "wore the mask of a man brought back from the tomb." Even in December he was like "a spectre of science."

It was not until January, 1869, that he actually began to lecture again. He said at the opening of his course at the Collège de France:

> We are about to resume our studies in experimental medicine which have been interrupted very much against my will for three years. I have reason to regret this interruption deeply; for this lacuna in my scientific life is precious time for ever lost; nevertheless perhaps you will see later that it has not been utterly lost. Unhappily for me I had all this time for reflection and I was able to use the leisure which my illness left me to organize my ideas and form projects for study which will not be without value in the course which we are about to resume. I have only one desire, that is, that my health, the present state of which will oblige me sometimes to appeal to your indulgence, may gradually mend and allow me still to serve that science to which I have devoted my entire life.

This service for the present was not to be carried on so much in the laboratory as in his public capacity as an eminent man of science. He had refused the year before the secretaryship of the Academy of Sciences but had accepted the vice-presidency, which, as was customary, led to his election as president for the next year, 1869. The Academy met every

[1] *Goncourt Journal*, 3: 200, 240.

Monday throughout the year and only once did Bernard fail to preside except when he went away on his annual holiday during September and October and left M. Chevreul, the director of the Museum of Natural History, to act in his place.

M. Chevreul was already eighty-three years old and he was destined to live to be one hundred and three. Bernard liked to visit him on Sunday afternoons in his rooms at the Museum, which in winter were extremely cold, heated only by "two small billets of wood propped up end to end in Buffon's ancient fireplace."[1] Chevreul's conversation had become a legend. The reminiscences of more than three-quarters of a century were mingled with philosophical speculation. Bernard was interested in M. Chevreul's conception of what constituted a fact and incorporated it into one of his lectures.[2] The great difficulty with the old gentleman's discourse, however, was that it was essentially a monologue, association of ideas leading him from one point to another as in a dream. Bernard had invented a technique for interrupting him in order to make a departure. He would interject a pun (he was somewhat addicted to puns) and while M. Chevreul paused to digest it he would ceremoniously make his adieux and escape.[3] He often met the Barrals at M. Chevreul's and the friends would walk away together rapidly, continuing the conversation and trying to restore their benumbed circulations.

The same year, 1869, saw the beginning of Bernard's political career which was, indeed, short but which occurred at an exciting moment in the history of his country. An Imperial decree of May 6 made

[1] Barral, G., *Préface historique* to *Arthur de Bretagne*. [2] xv, 40.
[3] Berthelot, M., *Mém. Acad. d. Sc.*, 47: 387, 1904.

him a senator. His yearly income was in consequence augmented by 30,000 francs and there is still among his relics at Saint-Julien a card bearing five stamps, which shows that he received five quarterly payments of his stipend. No historian can bring himself to say a good word for the Senate of the Second Empire. Its duties, which had been somewhat loosely defined by Napoleon III in 1856, were to watch over the general welfare of France and propose useful reforms in season. The consensus of opinion is that it performed no service beneficial to the country, but merely upheld an autocratic administration until it shared its downfall.

Bernard took his duties as senator seriously, as he did all obligations which he assumed. There is not a day's balloting recorded in the official journal after Bernard took his seat in August, 1869, which does not include his name. Although his attendance was exemplary there is no record of his having said a word. One would have thought that he would have been stirred to speak in the debate on liberty in higher education, in which French universities were compared with those in England, Belgium and Germany, or on the question of free medical service in rural districts; but the only occasion on which he is mentioned as taking part in the deliberations is when he was one of a number to bring in an amendment to the constitution relative to the choice of senators. The article as proposed stated that the Emperor should nominate senators after deliberation in council with his ministers; the amendment deleted this phrase, thereby removing any restrictions and giving the Emperor a perfectly free hand to choose anyone he pleased. That Bernard should sponsor this amendment is an excellent commentary on his attitude towards the

political principles exemplified by the Second Empire. It is not recorded that he shrank even from that aspect of his functions as a senator which demanded that he appear at public functions *en grande tenue, pantalon bleu*—in full dress, with blue trousers. At least he was spared the anxiety about what one ought to wear which, it is said, had assailed him before his first visit to Compiègne.

At the death of M. Flourens late in 1867, Bernard's friends had urged him to apply for the vacant seat in the French Academy. Not only had Flourens been a physiologist but his essays in scientific biography had been much admired. Bernard's literary reputation had been made overnight by the publication of the *Introduction to the Study of Experimental Medicine,* and he was regarded as a suitable successor to the chair of one who united the qualities of a scientist and an author. Bernard set about making the necessary calls upon persons of influence and was elected in May, 1868. It was not, however, until a year later that his reception into the French Academy took place. He seems to have had a great deal of difficulty over the preparation of his reception speech. He wrote it out again and again in his own hand, and because this manuscript, a loose pile of separate sheets in which the three or four different drafts are inextricably intermingled, was found long after his death in the attic of his house in Saint-Julien, we may perhaps infer that his holiday in 1868 was somewhat marred by the effort of composition. Even when the printed proof reached him he was still dissatisfied and covered it with interlinear corrections and rearrangements.

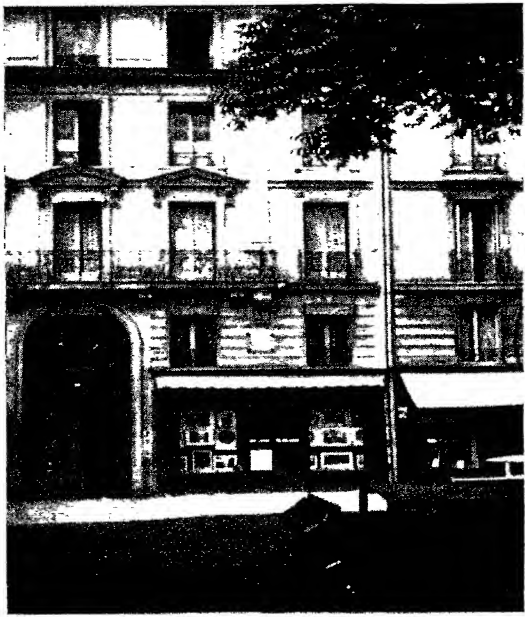
The greater part of his speech was taken up with the customary eulogy of his predecessor, Flourens,

and an account of the latter's work on the brain. Bernard used this work as an illustration of his own general position that the brain can be investigated by the same methods as any other organ of the body, such as the heart or stomach. He paid tribute to Flourens as an original investigator, but he was perhaps a little lukewarm in his appreciation of him as a writer. He described him as a "successful popularizer" and commended his gifts in scientific generalization, but we know with what suspicion Bernard habitually regarded any tendency in that direction. He concluded with a vindication of the experimental method, the exposition of which in the *Introduction* M. Patin[1] had praised earlier in the afternoon as having "created a new style."

From newspaper reports one gathers that the speech was not particularly well received. The audience remained cold and indifferent. Bernard had made no effort to be anecdotal or amusing, and while his many revisions of the text at Saint-Julien had resulted in a very finished speech, they had also robbed his style of the directness and spontaneity which had been so much admired in the *Introduction*.

There is one passage in the speech which has a special significance when it is viewed in the light of the events which were taking place at the moment in Bernard's domestic life. Flourens, Bernard said, "found in his own home the calm and repose so necessary for a hard-working scientist. His wife, devoted, well fitted to understand and appreciate him, identified herself with his intellectual life, which

[1] M. Patin was the director of the Academy who was in charge of Bernard's reception. The Goncourts extracted from this occasion a slightly *risqué* item for their *Journal*: "April 30, 1869. . . . At this moment, ridiculously enough, Claude Bernard was delayed in being received into the Academy, because Patin could not make his reply. The unlucky Patin forgets every day at the foot of the stairs, the physiology that the physiologist taught him in his closet."



40, RUE DES ÉCOLES, WHERE BERNARD LIVED DURING HIS LAST YEARS

CLAUDE BERNARD'S MOTHER WITH HIS DAUGHTER, TONY, FROM A PHOTOGRAPH TAKEN ABOUT 1850

PLATE IV

she enhanced by shielding him from the very cares of existence."[1] From what one may gather from the testimony of Bernard's pupils and friends this would serve almost as a portrait in reverse of Madame Bernard. The marriage seems to have been doomed from the beginning by the extreme incompatibility of the contracting parties, and its lack of success was notorious among Bernard's acquaintances. He was for the most part silent about it himself, although he seems to have confided in the Barrals. Edmond Goncourt relates that the novelist, Zola, at one time thought of Bernard as the prototype of his Doctor Pascal who was to have been represented as a "conjugal martyr."[2] The hero of the novel as we have it does not seem to have been drawn from Bernard, but at least his wife destroys the papers which embody her husband's scientific researches.

Madame Bernard was a strict Catholic and, in the opinion of her critics, she carried her devotion to the point of bigotry. She was intellectually very narrow and she was not only quite uninterested in her husband's scientific pursuits but actually antipathetic to them because of her horror of vivisection. She continually protested against it and subscribed to the French equivalent of the Society for the Prevention of Cruelty to Animals, entering these small sums in her household accounts where they may still be read. Later she and her daughters went so far as to found an asylum for stray dogs and cats. In the early years of her married life there probably were incidents which would make a person of her temperament feel justified in the attitude which she adopted. In all fairness, one must admit that it would be distressing to any housewife with one small child two years old

[1] xviii, 435. [2] *Goncourt Journal*, 8: 141.

and another on the way to have her husband bring to their small apartment on an upper floor on Sunday morning a dog having an open wound in its side from which internal fluids were drawn off from time to time, a dog in a state of extreme emaciation, yet with a voracious appetite, pus running from its nostrils, coughing as it was led up and down stairs and suffering from diarrhœa, its fæces being of particular interest to the master of the house. We have it on Bernard's own testimony that this happened in 1849.[1]

It is of no use to try to gloss over the fact that Bernard practised vivisection. The type of experiment to which Magendie introduced him may seem to some to have involved cruelty: the length of time which an animal can live without oxygen;[2] the temperature to which a rabbit in an oven must be heated in order for it to succumb;[3] the influence of pain on the heart-beat.[4] One must remember that Magendie's greatest achievement was to show that stimulation of the dorsal roots of the spinal nerves gives rise to pain. When Bernard began his experimentation there were no anæsthetics, and patients in the hospitals as well as animals in the physiological laboratories had to bear the pain of operations without alleviation. As soon as anæsthetics were introduced he began to use them and thenceforth his operations were performed painlessly.[5] He stated plainly: "If an illustration were required to express my feelings in regard to the science of life, I should say that it is a superb *salon* resplendent with light, which one can enter only by passing through a long and ghastly kitchen."[6] Nevertheless, although he regretted the disagreeable features of the "ghastly kitchen," he was

[1] vii, 387–8. [2] x, 45. [3] iv, 209.
[4] x, 324. [5] xv, 74. [6] viii, 28.

convinced of its necessity. He pointed out that the physiologist must experiment on higher animals while they are alive, since cadavers are useless for his purpose,[1] and results obtained on lower animals, such as frogs, are often not applicable to man.[2] He illustrated his position by a story from French history:

> In one of his campaigns the duc de Bourgogne, son of the Great Dauphin, wrote to Fénelon to ask him if he could have his army encamped beside a convent in spite of the inconveniences of such proximity from the point of view of morals, adding that the choice of this particular camping place was necessary for the success of his military operations. "Burn the convent, if you have to, but win the battle!" replied Fénelon. . . . Physiology has its indispensable and pressing necessities; one must submit to them or renounce all progress.[3]

He said on another occasion:

> The true, the only reason which we can give for vivisection is that it brings about the advance of science, that science has its practical applications and that by these applications—some actually realized, others yet unknown but not the less certain—practical medicine is enabled to help millions of human beings as contrasted with the few animals upon whom we have imposed an instant of suffering. These views are, unfortunately, not familiar to people in general, especially to those who recently have risen so violently against vivisection.[4]

The bickerings in Bernard's own household over this topic increased, and as time went on the husband came home chiefly at meal-times, and even then his aloofness and imperturbability only served to increase his wife's irritation and her recriminations. Bernard suffered an overwhelming disappointment in the

[1] x, 219. [2] xi, 545. [3] xi, 548. [4] xv, 74.

death of his last-born son in 1857. His two daughters were completely dominated by their mother. The items in the household accounts in 1865 which have to do with feminine pocket-money suggest the circumscribed and trivial interests of the mother and her daughters. There are small sums for articles of dress, sweets, religious tracts and pictures, donations for the *curé*, Easter eggs. There is a great deal of borrowing and meticulous paying back of tiny sums, all scrupulously recorded. Madame Bernard mentions paying Tony and Marie 25 centimes for the bite which she had taken out of their Easter egg. A few francs were laid out for "Monsieur" on white woollen socks and a flannel band. And all this triviality leaves the impression of having been completely absorbing. At Saint-Julien a tradition, perhaps not completely unbiased, took root and flourished that Mme Claude Bernard was *folle*.[1]

The joint accounts come to an abrupt end in 1866 when Bernard went alone to Saint-Julien to try to nurse himself back to health. Whether abortive attempts at life in common were made after his return to Paris is not quite certain. Early in 1869 we find him writing to the Mother Superior of the Maison du Sacré-Cœur countermanding the engagement of rooms for his wife and daughters since madame had declined to agree to his arrangements on the ground that she would not be able to take her

[1] It has been suggested by Anatole de Monzie in his *Les Veuves abusives*, p. 89, that the real source of Mme. Bernard's bitterness against her husband was the use of her dowry to pay off the debts left by Bernard's father at his death in 1847. M. de Monzie says that the loss of the dowry was the subject of constant recriminations on the part of Mme. Bernard, who is represented as being "*l'éternelle bourgeoise*," infatuated with the dowry which she brought to the marriage, unable to forgive her husband their straitened circumstances and lack of social position. At the time of Claude Bernard's death in 1878 there were newspaper stories that he had been brought into great difficulties by his assumption of his father's debts at a time when he was still working to establish his own career.

two dogs with her. In this year M. and Mme. Bernard definitely began to live apart and on April 22, 1870, a formal decree of separation was obtained, Bernard being in residence in the rue Luxembourg and Mme. Bernard in the rue Cherchemidi. Along with his share of the household goods Bernard asked for his children's pictures, some of their books, including one which the Empress Eugénie had sent as a gift to his daughters, and the playthings which had belonged to his younger son. That the death of this last child had been deeply felt by Bernard is borne out by a recollection of M. Tripier which is preserved in his letter to Professor d'Arsonval who has published it.[1] M. Tripier said that soon after he took up his duties as secretary the child's death occurred and Bernard confided that he "now understood the upheaval in the religious ideas of those who did not feel themselves upheld by an idea of another order" in such circumstances.

Madame Bernard with Tony and Marie withdrew to Bezons (Seine-et-Oise) and they were soon lost sight of by Bernard's pupils and friends. They reappeared for the last time in relation to Bernard when they brought suit against M. Barral after his publication of *Arthur de Bretagne* because of their objection to the following statement in his preface: "It is to the eternal honour of MM. Matthias Duval, Paul Bert, d'Arsonval [and a list of a score of other pupils and friends] that they did not neglect Claude Bernard in the cruel state of abandonment in which on a sad morning in 1869 his wife and his two daughters left him." They fought the issue before the courts and in 1889 won a judgment against M. Barral for 20,000 francs, together with a court order that all copies of

[1] d'Arsonval, A., *Méd. gén. fr.*, 2: 12, 1935.

the book, either at the publisher's or in any book-shop in France, were to be destroyed. Copies which had already been purchased were, of course, exempt, but that Mme Bernard was on the watch for these if they should chance to come on the market again is shown by a letter, which is still extant, written to her by a bookseller in 1893 to inform her that at a sale at the Hôtel Druot a copy of the rarity had been sold for 40 francs, and that he would sell her the two copies which he had for 50 francs.

Although the breach between Bernard and his wife was complete it would seem that affectionate relations between the father and his daughters were not entirely broken off. There has been preserved at Saint-Julien a letter written by Bernard to the elder, Tony, in 1874:

MY DEAR TONY,

I am sending you to-day a basket of pears; the ripest are on top, the less ripe underneath. You will put them in a cupboard: they will ripen in succession and be good to eat in a week or a fortnight from now. In a few days I will send you a basket of black and white grapes, and some peaches from the orchard; for the ones on the espalier are all over.

Your aunt Jenny and her children have come to spend a few days with me and have gone back to Pouilly. They send you their love and kisses. I saw M. Chrétien yesterday; he has lost his old Jeanne, whom perhaps you remember. He sends you his compliments.

It is very hot here, all the fruits are drying on the trees: the vintage will be better than last year, but it will still be a poor year.

I send you and your sister my best love.

Your affectionate father,

CLAUDE BERNARD

Saint-Julien,
September 7, 1874

In another letter, written to Mme. Raffalovich, whose friendship with Bernard began about this time, there is a passage referring to the year 1869 and its painful events. The strained language bears witness to the emotion under which Bernard was labouring:

> The year 1869 will have been for me a period of disquieting and painful change in which the most diverse events have occurred; vexations and honours have been heaped upon me simultaneously, and some gleams of hope have appeared like a vivifying ray of sunlight in a transient patch of clear sky between two dark clouds; but little by little, just as a storm subsides, the heavens take on the mournful, misty and monotonous sadness which henceforth awaits me in the autumn of life; illusions fall away from my soul one after another as leaves fall from the trees in the autumn of the year.[1]

[1] *Raff. Corr.*, 3653, 7.

CHAPTER SEVEN

THE RAFFALOVICH CORRESPONDENCE AND THE WAR OF 1870–1

Je vous remercie de votre lettre attristée sur le sort de la France. Je ne me croyais pas destiné à être témoin de tous les malheurs de mon pays qu'un odieux vainqueur peut maintenant parcourir sans obstacles et avec insolence.[1]

IN THE LIBRARY of the Institute in Paris there have been preserved almost five hundred manuscript letters written by Claude Bernard to Madame Marie Raffalovich between the years 1869 and 1878. More than half of these are merely short notes accepting or refusing invitations or announcing an intention to call when both correspondents were in Paris, but the rest are comparatively long letters written at such frequent intervals that they almost constitute a diary of the later years of the great scientist's life. The letters are an exception to Bernard's regular practice. He wrote to another correspondent:[2] "I am an 'epistolophobe' by nature and I have on my conscience hundreds, perhaps I should say thousands, of letters which I have never answered." Several of Bernard's biographers have remarked that he wrote very few letters, and all completely disregard the Raffalovich correspondence. As it happens, we have on record Bernard's opinion of the use of private correspondence for biographical purposes. He, with Chevreul and Barral, was intensely interested in the

[1] *Raff. Corr.*, 1870.
[2] M. le baron Giradot, Nantes, March 8, 1876: MSS. *du fonds Charavay, Bibliothèque de la ville de Lyon.*

publication of the correspondence of Napoleon I, and he said:

> It is in such correspondence, which certainly was not intended for publication, that one must study Napoleon. All these letters are acts which reveal the man a thousand times better than the contradictory judgments of his contemporaries. The Emperor could be belittled by such a publication. He actually emerges greater, more marvellous, more extraordinary, if that is possible. I shall have to tell Taine to delineate for us a Napoleon according to Napoleon's own testimony, and not according to the scraps of papers he sets himself to collect from all sides.[1]

Madame Raffalovich was a Russian Jewess from Odessa living in Paris with her family—her husband, her grown son, Arthur, who later became a leading economist, and two younger children. She was apparently talented, charming and well-to-do, but the original ground of the friendship between her and Bernard seems to have been a common invalidism; at least, the early letters are concerned with the recommendation of a physician, Bernard's solicitude about her health and confidences with regard to his own. The curve of the distribution of the correspondence by years rises to a maximum in 1873, when there were nearly two letters a week, and falls away gradually thereafter.

Madame Raffalovich was an accomplished linguist and an amateur journalist, contributing especially articles about foreign science, art and literature to papers in St. Petersburg. Very early in their friendship we find Bernard thanking her for help in his reading of "physiological-philosophical works," for résumés and for translations, particularly from the German since he was unacquainted with that

[1] Genty, M., *Biogr. méd.*, p. 151, 1932.

language. He trusted her judgment in literary matters, presented her with a copy of his *Report* and asked her opinion of it, and later he consulted her about "style" and "order of ideas," arranging to call and read her a revised communication for the Academy before he actually delivered it.[1] They discussed philosophical matters which interested him, for example, the definition of causality, and she helped him with a study of Leibnitz of which we can recognize some of the consequences in lectures of this period.

When she expressed a wish to attend some of his lectures he tried very hard to discourage her, warning her to sit near the door if she came, and recommending that she attend in preference Berthelot's course which was given at the same hour. He arranged to meet her at the conclusion of one or the other and begged her not to be impatient as he never finished exactly on the hour. She sent him invitations to her box at the theatre and very frequent invitations to dinner at her house. Occasionally she accompanied him to exhibitions of pictures by contemporary artists. With the vagueness characteristic of unscientific people about the boundaries of the sciences, she sent him botanical specimens to identify, which rather taxed his powers. At the height of their intimacy she overwhelmed him with gifts of a coffee-pot and a superb dressing-gown, magnificently lined with grey squirrel. He thanked her for her extreme kindness to one "constantly subject to bad colds"; and it must be admitted that the correspondence gives her every justification for such a gift, for, as soon as winter set in, Bernard reported one cold after another with occasional complications of neuralgia and laryngitis. However, the dressing-gown was almost too much for

[1] *Raff. Corr.*, 3655, 68.

him. His housekeeper, Mlle Mariette, thought it was too fine to wear, and, in real or mock seriousness, he asked Mme Raffalovich if she did not think he might have a winter overcoat lined with the fur while he kept the silk for a summer dressing-gown.

Bernard, on his side, sent his friend books on physiology which she was anxious to understand better, pamphlets (for example, his report for 1870 on the cholera prize), cards for public lectures and ceremonies, which, it must be admitted, she did not always make use of; and he called upon her assiduously at her house, very often making this visit the pleasant conclusion of an arduous day spent on committees and lectures. He took great pains over his compliments. Hers was a "sensitive and philological mind" and she possessed qualities which are "usually mutually exclusive, logical power and delicacy of feeling." And, for the reassurance of those who may have found this only alarming, he added that when a friend of his meets her he will see "a young and pretty woman with a superiority of mind the like of which he has never experienced before." One day, when he was feeling tired and ill and wished that his spirit had been housed in a better body, he said: "In your case, madame, there is complete harmony, the beauty of the spirit corresponds with the beauty of the body, that is why you are perfection for us all." A genuine master of the art with which Bernard was struggling might perhaps have omitted the last phrase.

Words, however, were not the only coinage of the friendship. He sent baskets of peaches and apples from Saint-Julien, game while he was in Paris, and he collected foreign stamps (*timbres exotiques*) from the wrappers of the numerous scientific reprints sent him

from abroad to give them to madame's two younger children, Mlle Sophie and M. Bébé. There are two letters, one in September, 1872, the other in the same month of the following year, which still hold, perfectly preserved between their sheets, the pressed violets which Bernard had enclosed. In the second of these letters he said: "Here are the first two autumn violets which have just blossomed at Saint-Julien."

There is nothing in the correspondence to suggest that Bernard ever took Mme Raffalovich directly into his confidence with regard to his domestic tragedy. It is true that he spoke of the year 1869, in a letter already quoted, as having been a painful one, but the expressions which he used are quite vague. When he refers at all to family matters, he speaks of his sister and her children and grand-children. His mother had died in 1867, shortly before the correspondence began. When Mme Raffalovich asked him to tell her about his native country-side and how he spent his time there on his yearly holiday he responded with enthusiasm:

Saint-Julien,
September 24, 1869

Dear Madame,

You have finished your excursion and I am continuing mine; for although I remain in one place, I can imagine that I am on the move, so far are my thoughts from their regular channels. And since you are so good as to ask me about the aspect of my country-side and how I spend my time, I shall reply to your kind inquiry; this will be my excuse for talking so much about myself.

You say that the trees should have made holiday on my arrival. To be sure, when one revisits one's native country, every bit of it is alive with old memories; but these memories bring me more sadness than joy. The cutting off of life has always been a thought filled with bitterness

for me and to-day more than ever. Nevertheless, I keep up appearances and pass for a very happy man. Beaujolais is situated on the right bank of the Saône. I live on the slopes that face the Dombe; I have for horizon the Alps of which I can see the white peaks when the weather is propitious. Beyond them, my imagination can travel as far as the Adriatic, and on this side of them my gaze passes over Geneva and Switzerland which are hidden from me by the Jura. But to remain within the limits of the visible, I see when I am at my windows or on my terrace the broad meadows of the valley of the Saône unrolling themselves below me for a distance of seven or eight kilometres. When in the morning, as for example to-day, the sun rises radiant behind majestic Mont-Blanc in the distance, the Saône and its meadows are covered with haze and a mist which gradually thins and dissolves. Then I can distinguish only the tops of the tall poplars and see the long snakes of smoke from the passing Paris–Lyons train, the distant rumbling of which I sometimes hear when the wind is from the east. On the hillside where I live I am surrounded by a boundless sea of vines which would give the country a very monotonous appearance if it were not interrupted by shady valleys and little streams which come down from the mountains towards the Saône. My house, although on a height, is surrounded by a nest of verdure, by a little wood on the right and an orchard on the left; this is a rarity in a country where even the bushes are uprooted to plant grape-vines.

The vintage has commenced since my arrival and it is not yet over. Every day I get up at six o'clock, I go down to the vats and superintend the fermentation and the pressing. During the day I go into the vineyards, oversee the vintagers while I inspect the vines. I have six vatfuls to make, which is the equivalent of about 160 or 170 barrels of wine; there are two already pressed, two in process of fermentation, and for two the grapes are still to be gathered. To-day I am having the grapes in the yard around the house gathered and as I write to you I

see the vintagers from my window. You see, my dear madame, that for the time being I am transformed into a winegrower. These are the occupations which are familiar to me and in the midst of which I was born; they always give me pleasure and they are certainly much more agreeable to me than composing academic discourses. The wine this year will be of excellent quality.

At the end of the month the vintage will be over. The mountain vintagers will climb back to their homes; the country will lose its animation and I shall be left in the midst of solitude. I do not dread it, quite the contrary. I have the whole month of October in which to surrender myself freely to tranquil reflection; for me that is supreme happiness. Then I shall sell my wines and leave for Paris to dive, as someone has said, into that abyss of vexation and misery, and so on to the end.

Thank you, madame, for the notes which you recently sent me. I have not yet had time to read them with sufficient attention, neither the first nor the last; but I have glanced through them and find them very illuminating and perfectly arranged. They will be extremely useful to me and I cannot tell you how grateful I am to you for them.

I am putting off the philosophical correspondence until after the vintage. Nevertheless I will tell you at once that it is *not* a morbid characteristic of your mind that the inexplicable always turns up, as you say, at the end of everything you write and think. Your mind is made in that way, and it is for that reason that it surprises and is unlike other minds. . . .

I hope that your health is good and your happiness complete in the midst of your family whom you cherish and who adore you.

Will you remember me to your husband and son and convey to them my affectionate good wishes.

Your devoted and affectionate

CLAUDE BERNARD[1]

[1] *Raff. Corr.*, 3653, 31.

After the vintage described in this letter Bernard returned to Paris and to the routine of his lectures at the Collège de France, which for the winter of 1869–70 were on anæsthesia and asphyxia. He sent the proofs of his introductory lecture to Mme Raffalovich. He was assiduous in his attendance at meetings of the Senate and of his Academies and he even went to a ball at the Tuileries and complained bitterly of how tired he was after it. The important academic occurrence of this year for him was his inaugural lecture at the Museum of Natural History, in which he reviewed the sequence of events which had led to the transference of his course in general physiology from the Sorbonne to the Museum in connection with the government's foundation of the new "*École pratique des hautes études.*"

This was in June. In July the Franco–Prussian war began. Mme Raffalovich was on the Normandy coast at Trouville. When the war news became grave early in August, Bernard wrote urging her to stay there. He in Paris was very restless and anxious. He was unable to work and he tried to distract himself with mechanical occupations like rearranging his books or preparing bottles for a marine collecting expedition which never came off. He even tried what he called the "heroic diversion" of going to see someone who had bequeathed him his body for experimentation after death. He was disappointed to find his would-be benefactor mentally unbalanced. He went continually to the Senate and he was there on the wild Sunday when the Chamber of Deputies was invaded and the Republic proclaimed following the news of the capitulation of Sedan and the capture of the Emperor. Before the end of the day he wrote to Mme Raffalovich:

Paris,
September 4, 1870

Dear Madame,

The newspapers will inform you of the events and the swift catastrophes which have plunged the country into an abyss. Invasion and revolution in its wake! . . .

I have just left the Senate, its last sitting doubtless: the legislative body was invaded with the old cry. . . . Long live the Republic! . . . The deputies were dispersed and could no longer deliberate. Delirium has set in . . . ![1]

These were anxious times. Even if his short political career had come to an abrupt close on the 4th of September, his scientific one went on. The Academy of Sciences did not falter in its stride. The 5th of September being a Monday, it met as usual and Bernard presented M. Jousset who read a paper on scorpion venom; but the war now began to occupy everyone's thoughts. The conversation of Bernard's colleagues at their regular bi-monthly dinner at the Café Brébant[2] on the 6th of September was of nothing except the war. Berthelot claimed that Paris was surrounded by enormous quantities of petrol which the Prussians had thrown into the Seine and which they planned to set alight so that a river of flame might devour both banks throughout the entire city. Renan, who had lost his professorship at the Collège de France but was to be reinstated later, was said to have shocked the diners by his stubborn insistence that the Germans were a superior race.[3] He denied having said this later when the *Goncourt*

[1] *Raff. Corr.*, 3654, 36.

[2] In honour of Sainte-Beuve, the leading literary light of the 1860's in Paris, the custom was established among the Parisian intellectuals of dining twice a month, beginning November, 1862, at the Café Magny. Renan and Berthelot were almost always present and the Goncourts recorded the conversation. Bernard was present only occasionally. After Sainte-Beuve's death in 1869 the diners transferred their rendezvous to the Café Brébant.

[3] *Goncourt Journal, 2me série*, 1: 25, 1870–1.

Journal was printed. In less than a fortnight his article on the war in the *Revue des deux mondes*[1] was to appear, in which he said: "I have always regarded war between France and Germany as the greatest misfortune which could happen to civilization." The article was essentially a plea for a European federation which would be superior to all nationalities. When Bernard read it he wrote to Mme Raffalovich that he thought it was well done, but that he disagreed with the author on certain unspecified points.

The war began to displace science as a topic of discussion at the meetings of the Academy of Sciences. A letter from the volunteer ambulances at the front was read in which amputations following war wounds were discussed. M. Charles Saint-Claire Deville regretted that observations and records from the meteorological station of Montsouris had had to be interrupted since the military authorities had obtained a requisition from the Minister of Public Instruction to use the building for the defence of Paris. Bernard presented a note by M. Rabuteau on a means of annulling the effect of insufficient diet by administering an infusion of coffee and tea with sugar to which powdered cocoa had been added. The author was sure that a man could live for several months by taking a few grammes of this mixture daily, and he expressed a hope that the Committee for the National Defence would see to it that the formula for this alimentary mixture would penetrate to the besieged cities. Food had become an absorbing topic and members of the Academy of Sciences were to debate for months the food value of products made by chemical treatment of bones. Dumas, Chevreul

[1] Renan, E., *Rev. d. deux mondes*, 89: 264–83, 1870.

and Milne-Edwards were to discuss gelatine as a food in meeting after meeting. M. E. Pelouze, the son of Bernard's old friend, was to discourse on methods of preserving meat, while M. Berthelot was to deliver paper after paper on the explosive force of gunpowder.

Bernard at first resolved to stay in Paris, but his agitation brought on a recurrence of the old abdominal trouble which he had hoped he was done with and he decided to leave the city on the 15th or 16th of September, his departure coinciding with the general exodus of non-combatants urged by the Governor of Paris as the Germans approached the city. The railway service was disorganized and he was compelled to spend the night at Bourges. Uncertain of the state of affairs at Lyons, he stayed with friends near Montluçon until news should come through. His health grew worse and it was not until the end of the month that he was finally able to leave for Saint-Julien. Once arrived he stayed there for the duration of the war and longer, in spite of abortive plans to return to Paris and invitations from Mme Raffalovich to join her and her family at Nice or in Switzerland. He felt some anxiety lest the Prussians should eventually reach even Lyons; and he mentioned that the desk upon which he wrote was his father's and that its handles had remained twisted and bent since the invasion of 1815. The mayor of Saint-Julien asked him if he did not know some of the doctors in the German army and if he would not put in a good word for his native village if occasion arose.

He was depressed at the thought that the development of science had failed to enlighten the world so that war between nations would be impossible;

instead, science in the hands of the German nation had become a brutal weapon. He did not seem to feel that his own country was in any way responsible for the outbreak of the war, but he was critical of what he called the lack of a "scientific spirit" in the governments of the day. He said: "The French nation has not sufficiently understood that it was necessary in our day to treat political questions as one resolves problems in science."[1]

He read the newspapers industriously and his comments upon the principal events of the war were made within a few days of their occurrence. He spoke of the fall of Metz which had taken place on October 28 and of the revolutionary disorders in Paris occurring at the end of that month in a letter written on November 5, and he mentioned the sortie out of Paris of November 28 in a letter of December 6. The language in which he expressed his patriotic indignation against the German invader, especially when his illness made him "see everything black," was thoroughly unrestrained. He spoke of the "odious conqueror" and the "barbarians from Berlin." He said: "When the Germanic monster which still dares to speak in the name of civilization has released its prey, after having ravaged her, trampled upon her and dismembered her, she will have truly perished."[2] Even certain German scientists whose work he had described before the war as "remarkable" and "of the highest importance" did not retain his respect. When Mme Raffalovich, evidently with the intention of bringing his thoughts back from the war to science, sent him translations from the work on nutrition of Voit and Pettenkofer and of Liebig, he dismissed their conclusions as "thin theories" and gave

[1] *Raff. Corr.*, 3654, 43. [2] *Raff. Corr.*, 3654, 39.

it as his opinion that their work had been greatly overrated.[1]

Bernard was very far from being an enthusiastic Republican, but he was moved to cry "Vive la République" with Gambetta at the end of 1870 because he approved so strongly of the latter's vigorous prosecution of the war. With the new year he thought he recognized a new phase of the struggle. He was encouraged by the stubborn defence of Paris and he himself felt ready to hold out "for another ten years" if necessary. He was so cheered that he passed on to Mme. Raffalovich a piece of local gossip about a native of Saint-Julien who had been captured. The prisoner-of-war in a letter written to the mayor reported that he was well and employed on a farm; the farmer's wife liked him and as her husband had left for the war and had not been heard of since, he hoped to take his place. Bernard seems to have been ready to sanction this plan for bringing about a peaceful fusion between the embattled nations.

He also commented upon the strong feeling in the provinces against Paris, a feeling with which he was not completely out of sympathy since he was, for the moment, a provincial himself. Many country people seemed even to be glad that Paris was suffering because they believed her to be the cause of all their political misfortunes. They were tired of submitting to the dictates of the capital where "a dozen suburbanites could overthrow the government and impose their will on all the rest of France." Bernard saw in this protest against over-centralization the promise of a thorough reorganization of France once the terrible war was over.[2]

With the signing of the political armistice on the

[1] *Raff. Corr.*, 3654, 46; cf. 3653, 22.

[2] *Raff. Corr.*, 3654, 50.

28th of January his briefly raised hopes fell. He was unwilling that France should lay down her arms while the invader was still on her soil. He wrote to Mme Raffalovich early in February:

> It is done, a shameful and disastrous peace has been signed. Although I might have been prepared for this unhappy dénouement, I was none the less most deeply depressed by the news. I walked up and down the whole day long with a feeling of distress which forewarns me of a fresh crisis.
>
> This then is the pass to which France has been brought by the negligence of the Empire, the ineptitude of the Republic and the odious hypocrisy of Prussia.[1]

In the moment of defeat Bernard recurred to his idea that science had failed to penetrate to politics. He felt that the real reason France had lost the war to Germany was the lack of "scientific sense" in the French masses. He pinned his hope for the future to the scientist and he trusted to the application of the scientific method to solve the problems of government. He was convinced that the scientist alone could eradicate from the mind of the populace the false belief that anyone who could strike off a telling phrase was a great statesman or a great soldier. He went so far as to prophesy that France's future avenger would be the scientist.[2]

All this was doubtless a confession of faith, but its convincingness is a little marred by our recollection of the career of one great scientist in the Senate of the Second Empire. His scientific colleagues may or may not have been equally confident that the destiny of France lay in their hands, but at any rate they shared his deep resentment over the indignities which their country had suffered. The aged Chevreul had

[1] *Raff. Corr.*, 3654, 54.

[2] *Raff. Corr.*, 3654, 54.

read a declaration before the Academy of Sciences protesting against the bombardment of the Museum of Natural History. Pasteur, when he heard of Chevreul's action, supported his protest by sending back to the University of Bonn the diploma for his honorary doctorate in medicine in that university on the ground that it bore the signature of King William of Prussia. The Dean of the Faculty at Bonn replied to Pasteur rebuking him for the insult which he had dared to make to the German nation in the Sacred Person of its sovereign, and Bernard, in referring to the incident, did not hesitate to approve the patriotism of the Dean at Bonn because he thought that every country should out of self-respect uphold the dignity of its official representative; at the same time, he suspected that the Dean and his compatriots in their hearts agreed with Pasteur that the Sacred Personage in this case was "an old beast who has been persuaded that he wins all the battles."

During the February armistice Pasteur, who had left his country retreat in the Jura to try to find his son in the army of the East and had then come to stay with his brother-in-law at Lyons, made the short journey up to Saint-Julien to call on Bernard. At the time of this visit Bernard was greatly perplexed as to whether he should try to return to Paris in spite of the disorganization of the railroads or await the end of the armistice. He learned that although a stairway had been destroyed in the Collège de France and the fourth floor of the house in which his apartment was situated had been struck by a shell, neither his laboratory nor his living-quarters had been actually damaged; on the other hand, he was disturbed by the rumour that absent professors had been put on the index and their salaries provisionally suspended.

When the Assembly which had been elected to make peace with Germany finally accepted her conditions at the beginning of March, Bernard expressed disgust both with the peace and with "the unprincipled government which had made it." He was convinced that the French would never pay the huge indemnity demanded. Mme Raffalovich returned to Paris for a short time early in March but Bernard still lingered at Saint-Julien. The revolutionary activities of the Parisian populace caused her to leave the city once more by the beginning of April, and he entirely gave up the idea of returning until the disorders in the capital had abated. He criticized the Republican government for its inability to deal with the situation, and when some of his letters were delayed in the post he was worried for fear they had been intercepted and would be judged compromising by government officials. He stayed on another month at Saint-Julien, declining an invitation to travel in Germany with Mme Raffalovich on the ground that a French scholar would be unwelcome, and early in June he at last returned to Paris.

He seems to have resumed immediately his regular routine, almost as if the war had never happened. The French Academy elected him to its chancellorship and he wrote to Mme. Raffalovich in Heidelberg that he would be "nailed to Paris for the next three months by courses and public functions." His last reference to the war is when he arranged to accompany Mme. Raffalovich in October to the function celebrating the reinstallation of the glass broken in the bombardment at the Jardin des Plantes.

CHAPTER EIGHT

LAST YEARS

À l'automne de la vie les illusions se détachent de l'âme les unes après les autres, comme les feuilles tombent des arbres à l'automne de l'année.[1]

AN AFTERMATH of the war of 1870–1 was the foundation of the French Association for the Advancement of Science. The idea began with a group of Alsatian French scientists living exiled in Paris. Inspired by a patriotic desire to restore the prestige of their country, they took the first steps towards forming a society with the popularization of French science as its object. They were following the example of Great Britain and the United States of America where societies of this nature had been in existence for many years. Claude Bernard was elected as the first President in January, 1872, a natural tribute to his eminence in his own science and the respect in which his name was universally held. Unfortunately, his health did not permit him to be present at the first session in Bordeaux in September, but in subsequent years he took an active part, presiding over sections, reading papers,[2] conducting demonstrations or participating in discussions. The Association met in a different provincial city each year and attendance at its meetings involved travelling, to which Bernard had a rooted objection. He was present, nevertheless, in 1873–5 and 1876 at Lyons, Nantes and Clermont-Ferrand, but he managed to decline Lille and Le Havre.

[1] *Raffalovich Correspondence*, 1869.

[2] "La chaleur animale," *Session de Nantes*, 1875, 18: 213–17. "La sensibilité dans le règne animal et dans le règne végétal," *Session de Clermont-Ferrand*, 1876, 18: 218–36.

He had always shown a marked reluctance to being taken out of Paris in any direction except that of Saint-Julien. Mme. Raffalovich on several occasions had invited him to join her family party when she was travelling, but only once, in 1875, did she succeed in getting him as far as Trouville. In 1869 he had rejected Brittany as being too far away and he once admitted never having seen Montpellier, although it had such celebrated medical associations, except from the train. In July, 1873, he made a little excursion to Boulogne with Pasteur. Never at any time did he set foot off the soil of his native France.

During the last six years of his life Bernard was as active scientifically as he had been in his most productive period twenty years before. He managed in spite of distractions to spend working hours in his laboratory, carrying out experiments with his own hands. He mentioned to Mme Raffalovich an explosion in the laboratory in which he suffered first degree burns in 1872 and two years later he was bitten by a poisonous animal in the course of an experiment. By this time, however, he had gathered about him a group of disciples, many of whom had at one time or another served him in the capacity of assistant or *préparateur*: Paul Bert, Gréhant, Ranvier, Malassez, Armand Moreau, Dastre, Picard and, later, d'Arsonval—and to these he could delegate some of the experimental detail arising out of his investigations.[1] The scene depicted by Lhermitte in his painting (executed in 1890 and now hanging in the Sorbonne) belongs to this period. Bernard in a laboratory apron is operating on a rabbit, and half a dozen disciples and the *garçon de laboratoire* are gathered around the table at one end of which Dastre is taking

[1] e.g., xii, 460 (Gréhant); xvii, 543 (Dastre).

notes of the experiment. Nevertheless, in spite of all this activity, little was added to Bernard's discoveries that was startlingly new. His work on animal heat, on diabetes and glycogenesis, on asphyxia and anæsthesia had been begun years before. He was able, however, to add much fresh material from his more recent researches to his treatment of these subjects in his lectures at the Collège de France during this period, and in their published form the later courses of lectures have greater unity and comprehensiveness than the earlier ones. This is particularly true of the courses on diabetes which belong to the years 1873, 1874 and 1877.

The audience which presented itself at these lectures at the Collège de France was a rather mixed one. First there were the disciples and scientists of established reputation, ranged in the places nearest the long table upon which experimental demonstrations were carried out. There were some students—Bernard was pleased when his audience looked youthful; and there were the amateurs, in whom the lecturer seems to have taken a surprising interest. The Emperor of Brazil, then visiting Paris, asked permission to attend the winter course of 1873 and his photograph is one of the relics preserved at Saint-Julien. In the same year two Dominican monks were very conspicuous at the top of the amphitheatre. Their ritual costume caught Bernard's eye and he mentioned them to Mme Raffalovich as "probably bewildered, but motionless from the beginning to the end." He was, of course, unaware that one of them would become the well-known Père Didon who included among his activities popular lectures on the reconciliation of faith and science. The lady auditors were rather distracting. Bernard expressed relief in 1873 when there was only one in attendance.

Nevertheless, he described them all carefully to Mme Raffalovich. There were the *habitués*: the elderly one with spectacles, the glowing Englishwoman, "as splendid as the weather," and the Spaniard with untidy hair. Later there was a blonde Russian doctoress accompanied as far as the door by her husband and a striking brunette whose escort sat through the lecture; and, most overwhelming of all, a very elegant lady, high up in the amphitheatre at the end of a row, with a bracelet of precious stones clasped around one ankle. Bernard could not keep either his eyes or his mind off this decoration and mistook the aorta for the carotid artery in the experimental animal. He put the blame for his awkwardness upon his bad cold and covered his confusion by coughing and using his handkerchief. The lecture was extended a half-hour beyond its normal limits and even then he had not said the half of what he intended.

Bernard's lecture style was never formal. Dr. E. Callamand, who attended the courses from 1875 through 1877, said that although Bernard wrote well he was definitely not an orator. He spoke without notes and without prearranged plan. The lecture was a "chat interspersed with many experiments, an extension of the laboratory."[1] Even Georges Barral, who was inclined to partisanship, described the lectures as beginning lamely although they became more animated and forceful as Bernard warmed to his subject. There is no doubt, however, that the lectures were impressive to the right sort of listener.

In the winter of 1873–4, a young *externe* from the Hôpital de l'Enfant Jésus came out of curiosity to listen to the lectures on blood and blood sugar, the topic which had been chosen for this series. He was

[1] Genty, M., *Biogr. méd.*, p. 142, 1932.

completely captivated and seized an opportunity which arose of introducing himself to the lecturer. Bernard had attempted to demonstrate the difference in temperature of blood in different parts of the body, but unfortunately his galvanometer, which was essential to the experiment, was out of order and the demonstration had been a failure. After the lecture was over and Bernard was execrating the behaviour of the instrument to the auditors who had gathered around the operating-table, the young medical student came forward and asked permission to examine it. He saw at once what was wrong with it and with skilful hands repaired the damage. Bernard was struck with the young man's practical knowledge of electrical equipment. He invited him to cross the road to his apartment for lunch. There he discovered that his guest was the son and grandson of country doctors; moreover, that his father had been voluntary assistant to Laënnec who had had Bernard's chair before Magendie. The entire afternoon was spent in conversation, with the result that Bernard asked the young *externe*, who was, in fact, at the time only twenty-three years old, if he would care to come to the laboratory and prepare the experiments which demanded the use of electrical instruments. This was the turning point in the career of d'Arsonval. As Bernard had been chosen to assist Magendie and had been turned from the practice of medicine towards experimental physiology and the chair at the Collège de France, so now d'Arsonval was chosen by Bernard to act as his assistant and was destined to abandon a medical career and to proceed to this same chair.[1]

[1] While Brown-Séquard, Bernard's immediate successor, held the chair of medicine at the Collège de France, d'Arsonval was still associated with the name of his master, for his laboratory for electrical experiments was situated on the rue Claude Bernard.

D'Arsonval's father was, however, somewhat upset over the proposed change in his son's plans and Bernard attempted to reassure him in the following letter:

40, rue des Écoles,
July 6, 1876

MONSIEUR,

You express in your letter feelings of which your son, Arsène, has already informed me. I know that you were counting on him to be with you and to relieve you at the end of your medical career, and that it is a great sacrifice to be separated from him in order that he may enter scientific life in Paris. I understand, monsieur, and I fully respect the natural conflict which arises in the hearts of a good father and a good son. All that I can say to you is that, for my part, ever since I have had your son with me, I appreciate him more and more.

I have seen few young people so well endowed as he for the cultivation of the sciences. He has a great fund of knowledge, a most inventive mind, taste and enthusiasm for both practical and theoretical questions and, added to this, a kindly and helpful nature which makes him loved by all his comrades and everyone who knows him.

You understand, monsieur, that it would be very difficult in these circumstances for me not to encourage him and not to believe it my duty to give him my affection and my support in the direction in which I believe he is destined to succeed.

Without doubt scientific careers are not always so rapid in their material results as a professional career, properly so called; but they have other pleasures which compensate. Besides, it is not a matter at this time of turning your son from his medical studies; quite the contrary. We have just finished my course at the Collège de France and your son will have his time almost free until the month of December. I have urged him to pass his medical examination.

Your son is still so young that he has time for reflection

before making a definite decision, but for my part I shall always encourage him in the direction of a scientific career, where I believe, as I have already said, that he has a fine future in reserve for him.

Believe me, my dear confrère,

Most sincerely yours,

CLAUDE BERNARD[1]

We have seen that about the year 1858 Bernard thought of perpetuating his ideas on experimental medicine in a treatise of which the introduction only was published as he planned. The first part of the main body of this treatise was to be a description of operative procedures and therefore essentially anatomical; the second part was to deal with the explanation of phenomena, that is, with physiological processes; and the third part was to treat of pathology and therapeutics.[2] His long illness and his enforced retirement to the country during the Franco-Prussian war made him realize that he would never carry out his intention. He was unwilling to abandon the scheme altogether, for he wished to "show the unity and continuity of his researches,"[3] but he felt that it would have to be modified. He decided to begin by publishing some of his earlier lectures on experimental pathology, disregarding for the time being the logical order of the original plan. These were the lectures which he had delivered in 1859–60 and which were now translated back into French by Dr. Benjamin Ball from the English rendering which he had published some ten years before. To these were added a set of lectures on the spinal cord and a selection of others on general topics, usually the opening lectures of courses, and the whole appeared in 1871 under the perhaps rather misleading title of *Experimental Pathology*.

[1] Godlewski, H., *Méd. gén. fr.*, 2: 16, 1935. [2] xv, v. [3] xi, vii.

Bernard was firmly convinced that there is no essential difference between the methods of physiology and pathology, that just as we can induce physiological changes by artificial means in order to study them, so we can induce and study pathological changes; and this is his real thesis. The content of the lectures expressly designated as experimental pathology might almost be described as experimental pharmacology, for he had recourse chiefly to the action of poisons, carbon monoxide, curare, etc., in illustration.

Two years later he began to construct a similar volume on operative physiology. In this there were to be illustrations of instruments for use in special experiments, diagrams of dissections showing how certain operations should be performed, etc. He was meticulous about the wording of the legends and the lettering of the figures, and he even held up chapters in order to do a few more experiments upon a point in question. Work on this volume went on for nearly five years, and at the time of his death Bernard had corrected only twenty lectures. Matthias Duval, who was then acting as his secretary for lectures at the Collège de France, undertook to finish the task. D'Arsonval aided him in the part dealing with the determination of temperature in different parts of the body by means of thermo-couples; and for the last nine lectures M. Gaston Descaine, an *interne*, translated back into French more of Dr. Ball's English notes of 1859–60, those on the digestive tract. The volume was published the year after Bernard's death. Thus, although we have not the treatise as originally planned, we have a substitute for it if we take the *Introduction* as Volume I, *Operative Physiology* as Volume II and *Experimental Pathology* as Volume III.

Bernard's first course at the Museum of Natural

History in 1870 had not been immediately reported in the *Revue scientifique* as his lectures usually were because of the interruption resulting from the war. Dr. Lemaistre, who had made the résumé, died in the ambulance service. In 1872 the lectures at the Museum were resumed and henceforth Dastre was in charge of preparing them for publication. The two volumes of Bernard's works entitled *The Phenomena of Life Common to Animals and Plants* do not give a chronological record of his lectures at the Museum from 1872 on. Rather they open with the last course which he gave, that of 1876, which was a discussion of the general principles which he had deduced from data cited in previous courses. These lectures begin with a consideration of life in its relation to environment, in connection with which is introduced Bernard's conception of the "internal environment" in higher organisms. They proceed to a characterization of life itself. The emphasis in the first volume is on the unity underlying all manifestations of life whether in plants or in animals. In both kingdoms all phenomena can be classified either as phenomena of vital destruction or of organic synthesis. The former category is briefly developed in accounts of fermentation, combustion and putrefaction, and, in a similar manner, organic synthesis is disposed of by discussions of the cell theory, chlorophyll, irritability and morphogenesis. Bernard had intended to proceed to the development of both categories in detail in two more volumes, and he had planned experimental work along these lines as an accompaniment to his lectures now that his laboratory at the Museum was in operation. It is undoubtedly to this projected work that he refers with a note of premonition in a letter to Mme Raffalovich written in the spring of 1876. He said:

I began my course to-day and at the same time I am drawn towards the preparation of my large treatise in three thick volumes of which I hope to begin the publication very soon. I shall probably be absorbed in it for the rest of my life, for now my days are numbered, and I am no longer certain of bringing my enterprises to a conclusion, at least of becoming a second Chevreul. . . . I think that I shall soon be freed from many of my extrinsic occupations so that I may regain possession of myself and give myself up wholeheartedly to my work, if my health permits. I shall be forced to cloister myself this winter and completely renounce society, if not my friends.[1]

As he had feared, he was interrupted by death. Almost his last task was the correction of the proofs of the first volume of the *Phenomena*. Dastre collected lectures on the origin of sugar, on respiration and on digestion and published them as the second volume.

Bernard's lectures, his winter and summer courses at the Collège de France and his spring course at the Museum, kept him occupied from December until August. The letters written to Mme Raffalovich suggest that he suffered from an almost continuous series of colds and attacks of neuralgia, but he always said, in cancelling an engagement to call or dine, that he would give his lecture the next day nevertheless. In July, 1875, he speaks of having risen and gone to his lecture fasting after having spent the previous day in bed. On another occasion he has recourse to a carriage to take him to his lectures during an attack of sciatica which made it painful for him to walk. Whatever his other duties or distractions the lectures always appear as the principal *motif* of his day. Here are some typical days:

[April —, 1872.] I wanted to write to you this morning,

[1] *Raff. Corr.*, 3658, 7.

but I had to go to prepare experiments for my lecture, then give it, then receive the Scandinavian scientists who were present at it, talk with them and show them experiments, and so the day passed. Now I am home again to prepare my Friday lecture, because to-morrow, Thursday, I shall be busy from morning to night with affairs, committees, academic meetings and a visit to 19, avenue de la reine Hortense.[1]

[June —, 1873.] To-morrow I have a very full day without counting the unexpected. I shall go at noon to the Ministry to lend my voice for M. Renan who, I hope, will be nominated to replace M. Saint-Marc Girardin on the *Journal des savants*. From there I shall go to the French Academy to nominate M. Viel-Castel. After which, dear madame, I shall direct my steps, if you will permit me, towards the avenue de la reine Hortense, and I shall have earned my visit.

[July —, 1875.] Yesterday in the morning I gave my lecture. After lunch I spent two hours in my laboratory. Thence to the Ministry to preside over the committee on subscriptions until five o'clock. From there to the Pont Royal to take the boat to go to dine at Sèvres. Returned thence, at half-past nine to the Opéra to see *The Huguenots*. Home again at 40, rue des Écoles at half-past twelve. I must add that my mind has not been less active than my body throughout this day of peregrinations.

Since his separation from his family Bernard had lived just across the road from the Collège de France at 40, rue des Écoles in a small flat on the mezzanine. He preferred to receive the members of his "scientific family," his most frequent visitors at this time, in his bedroom which his elderly housekeeper, Mlle Mariette, kept in an order so precise as to be almost conventual. In the far corner of this room was the bed with blue damask curtains, to the left as one entered

[1] Mme Raffalovich's house.

was the fireplace and beside the bed was a large armchair in which Bernard sat wrapped up in his dressing-gown, "a cap on his head which he would take off from time to time as he talked with a gesture which was characteristic of him, as if his thoughts were cramped by it. Near him, opposite the fire, was a little square table where the lamp was set in the midst of a mountain of reviews, pamphlets and new books which had been sent him from everywhere."

M. Jousset de Bellesme, to whom we owe this description, goes on to say that Bernard at this time greatly disliked reading and handed over to him recent scientific literature in order that he might abstract and report on it. Mme Raffalovich also made herself useful in this way, especially when translation was required. After his death Bernard's library was found to contain 1,275 volumes. Of these only Champfleury's *Balzac* and Brillat-Savarin's *Physiology of Taste* could be classed as literary; the rest were strictly scientific.[1]

M. de Bellesme adds that if Bernard disliked reading he liked writing still less. The devices to which he had recourse to avoid the direct labour of composition in the preparation of his published works have already been recorded, but he could not escape completely and the effort of writing became increasingly irritating as he grew older. He said in 1873:

> I am not out of my preface or rather I cannot get into it. I prefer doing experiments. I notice in this connection that one takes one's habits of mind in relation to the nature of the things one studies and the way in which one studies them. The experimenter cannot think without being stimulated by an experiment, by a natural fact which presents itself to him; the mind grows dull and always

[1] Godart, J., *Les Réliques de Claude Bernard à Saint-Julien*, p. 34.

> needs an exterior excitant. The thinker properly so called takes his initiative in himself, and facts sometimes annoy and embarrass him. But all these reflections do not write my preface.[1]

Despite this distaste which he professed for writing on abstract subjects he nevertheless voluntarily undertook the labour occasionally, and when he did so he was extremely anxious that the expression of his views should be exact. He said: "Never do I form my opinion on that of others; on the contrary, I always try to conceive of things as I feel them, and it is not until after I have done this that I read what has been written on the subject.[2] In 1872 he published in the *Revue des deux mondes* an article on "The Function of the Brain" in which he developed the position which he had outlined in his discourse at his reception into the French Academy. He contended that the phenomena of consciousness and intelligence were merely "the result of the functioning of the organ which expresses them," that this organ was dependent upon physiological conditions and that its functioning could be investigated by the same methods as those used in the case of any other organ of the body. It is interesting that he anticipated Pavlov's method in the latter's work on conditioned reflexes by citing an experiment whereby saliva can be collected from the parotid duct of a hungry horse when the experimenter makes a movement towards the animal's feed-box. The publication of the article created something of a stir, upon which we have Bernard's own comment:

> You were quite right, dear madame; my article on the brain gave rise to a variety of appreciations. Yesterday at the Academy I had compliments with various implications. The Voltairians and free thinkers seemed satisfied;

[1] *Raff. Corr.*, 3656, 3. [2] *Raff. Corr.*, 3653, 32.

> the others find that I avoid committing myself. To the latter I reply that this is the greatest praise they can bestow upon me. I apply the scientific method to these questions; I am by intention neither materialist nor spiritualist. I give small thought to where the truth will lead me provided that I find it. I shall have much to tell you about all this.[1]

He made a very definite attempt to state his philosophical position in an essay, *The Definition of Life*, which was published in 1875. He tried hard to clarify his own ideas before committing them to paper. He wrote to Mme Raffalovich: "I have my impressions but I wish to understand myself in order that I may make myself understood by others. I explain my ideas on the properties of physical, chemical and vital matter to everyone I meet; yesterday to M. Chevreul, to-day to M. Berthelot, another day to someone else, and so on."[2]

His agitation at this time got him up at five in the morning to write a preface and kept him up until after midnight working on "abstruse metaphysical-scientific questions." His conclusions found their way both into the essay, *Definition of Life*, and into the beginning of his course at the Museum for the following year, 1876. The only change from the position taken up in similar essays ten years before was a tendency to emphasize the importance of the *unity* of the organism while still maintaining the possibility of its ultimate physico-chemical analysis.

A literary project which, according to Georges Barral, occupied Bernard at intervals during the last three years of his life was an account of the physiological opinions of the encyclopædist, Diderot. The initiative seems to have come mostly from the side of

[1] *Raff. Corr.*, 3655, 9.

[2] *Raff. Corr.*, 3657, 54.

Barral who was in the habit of coming to 40, rue des Écoles from nine until twelve every Sunday morning to read aloud to Bernard. The chief difficulty lay in the choice of a book. Barral tried poetry, both ancient and modern, but Bernard was not successful in concealing his boredom. Finally, Barral found common ground for literature and science in Diderot and his *Physiological Notes.* Bernard was interested, and the idea occurred to Barral that they might collaborate in publishing an account of Diderot's opinions on this subject. Barral took notes of Bernard's comments as he sat, listening attentively, in his corner by the fire in winter, or by the open window in summer. More than a third of the material was gone over in this way, but the work was never finished. It was perhaps out of gratitude for these attempts to cheer his lonely old age that Bernard presented Barral with the manuscript of his play, *Arthur de Bretagne,* in 1876, one day in August, after he had given what proved to be his last lecture at the Museum of Natural History.

During these years Bernard maintained his custom of going down to Saint-Julien in August or September and staying through October. He took with him his *cuisinière,* Mlle Mariette, who stayed until the last week and then went back to Paris by way of her native Auvergne where she took a brief holiday, Bernard in the meantime being cared for at Saint-Julien by his niece. It was at Saint-Julien that he received his sister and her daughter and grand-children. He was particularly attracted to his grand-niece who in 1875 was three years old. He described her to Mme Raffalovich. She was gay, never cross, and had a little roguish face. He was delighted, too, with her curiosity which led her to criticize or want to

find a reason for all that she observed. She could not understand, among other things, why seeds had been put in grapes; they were not only useless from her point of view, but very unpleasant on the tongue. It was into the possession of this grand-niece that Bernard's own house at Saint-Julien eventually came.

He still occupied himself with overseeing the vintage every year and he had other country diversions. In 1875, when he was sixty-two, he mentioned the revival of a pastime of his youth, the snaring of titlarks. He rose at dawn, concealed himself in some bushes and by imitating the call of these birds and manipulating a mirror decoyed them within range of his gun and so filled Mlle Mariette's casserole. It would appear that titlarks (they were regarded as a delicacy by the Emperor Caligula) are small migratory birds, feeding on figs and grapes, that they are very succulent and that advantage is taken of their fancy for looking at themselves in a mirror. When the vintage was over there was the local fête, which was subsidized by the income of fifty francs a year from a legacy left by an old labourer on the condition that a mass was said for him on the morning after the celebrations. Bernard declared himself as having been more honoured than entertained by the serenade under his windows which was part of the festivities in 1874.

When he wrote from Saint-Julien Bernard was very often apologetic to Mme Raffalovich about his failure to get on with his philosophical reading. He said in September, 1873: "I brought Descartes and Leibnitz down here and I shall perhaps not do them the honour of opening them; the country makes me intellectually indifferent." There is, however, at Saint-Julien, in addition to the note-book in which Bernard recorded experiments (mostly on curare), another in

which he abstracted a contemporary history of philosophy and the first volume of August Comte's *Lectures on the Positive Philosophy*, with some incidental comments. He was much more genuinely interested, however, as will appear later, in some experiments on fermentation which were under way in 1873. He wrote in October of that year: "Although I have done nothing (in the way of reading), I have obtained some interesting results on vinous fermentation."

Back in Paris, besides going to dine with Mme Raffalovich and with the Renans, he went to the Baronne de Rothschild's (from whom he also received gifts of game) and he was an occasional guest at the Princesse Mathilde's as late as 1876. In 1874 he was noticed there by Edmond Goncourt because of the part he was taking in the conversation. He was talking about the disuse of bleeding as a method of treatment in the hospitals. Formerly one had been able to procure blood by bucketfuls, but Bernard now had difficulty in getting enough for his demonstrations. He knew only one old doctor who kept up the tradition and even applied it to himself, with the boast, "I bleed myself every day in the week and water my flowers with it." Goncourt was once more struck by Bernard's appearance. He said: "Claude Bernard is interesting and delightful to look at. He has a fine head, that of a good man, a scientific apostle. Then, when he speaks of his own discoveries, he has such a distinguished way of saying, 'One has discovered . . .' "[1]

During 1875 Bernard made fewer complaints of ill-health than in the immediately preceding years. He seems to have gone out a great deal. We find him in August dining in the boulevard Haussman with two

[1] *Goncourt Journal, 2me série*, 5: 163, 1872–7.

retired business men who collected pictures. Bernard was interested in their Riberas and Murillos. M. Tripier has recorded that Bernard from the time of his first arrival in Paris as a student when he visited the Louvre was always interested in painting and sculpture. He did not care so much for music, but he regretted not having more time to devote to the Salons. We know that he attended the posthumous exhibition in 1872 of Henri Regnault who had been killed in the Franco-Prussian war, and that he had been interested in this young artist even earlier. M. Tripier says that when Regnault first began to be talked about, his father, the well-known physicist, brought to the Institute some of his son's sketch books and Bernard was so delighted with them that he borrowed them and kept them for a week. There is no indication that he was aware of the beginning of the Impressionist movement just at the end of his life. His appreciation of style is shown in his comparison of the graceful form of the frog with the statuary of Canova and the rugged musculature of the toad with the sculpture of Michelangelo.

As 1875 passed into 1876 Bernard's health began to trouble him again. He wrote in February, 1876: "I gave my course yesterday with great difficulty; to-day I am exhausted in body and mind: it is a new edition, with amendments and augmentations, of my cold in December. I must give in. I shall not have the pleasure of dining with you to-morrow. I am staying at home beside the fire." It is rather sad that towards the middle of 1876 even Mme Raffalovich allowed him to feel neglected. He wrote her a very melancholy letter in July suggesting that he was on less confidential terms with her about his scientific interests and that she had transferred hers to the camp of the

littérateurs. He said that he thought that "age creates emptiness around us." A few days later he wrote in genuine distress, for Mme Raffalovich had, for the first time since their friendship began, left Paris without informing him of the day of her departure in time for him to call and make his adieux. Their correspondence was resumed, however, when he went down to Saint-Julien and there was even talk of madame's daughter, Mlle Sophie, visiting him there; but in 1877 there was a fresh misunderstanding. In an undated note he wrote:

> If I have had a moment of irritability or of vivacity, I beg pardon for it. It was not for the reason which you doubtless suppose. I feel that I have some explanations to give you myself on this subject. I shall come to see you on Sunday evening between eight and nine on my way to make my adieux to the princess, your neighbour.

He went down again to Saint-Julien and during this last vacation he was very busy with his experiments on fermentation. When he returned he brought with him a collection of flasks enclosing the juice and must of grapes. He asked d'Arsonval to examine these for alcohol and for ferments, for he thought that he was on the track of a soluble alcoholic ferment. He gave special instructions that no one was to be informed of these experiments, for he evidently did not wish to hurt Pasteur.

Bernard had always been sympathetic with Pasteur in his struggle to gain recognition for his theory of the microbe origin of disease. D'Arsonval was present at a conversation between the two which shows Bernard's attitude. After having been heckled at the Academy of Medicine by Collin d'Alfort, Pasteur went to the Collège de France to tell Bernard of the stormy scene which had just taken place. He was so discouraged that he ended his account with the words, "Dear

master, do you sincerely believe that any of my work will survive?" Bernard replied with smiling good humour:

"Have no doubts about it, my dear friend; first of all, there will remain your experiments which cannot be attacked and which, besides, have already made converts for you; now, only a few days ago, two surgeons came to give me a cystic examination. The first was our confrère, Gosselin, the second, young Félix Guyon, who is imbued with your methods. Well, I noticed that both of them washed their instruments and their hands. Gosselin washed his *after*, but your pupil, Guyon, *before* this small operation."[1]

Bernard began his winter course at the Collège de France as usual, but when Georges Barral led him across the street on December 28, 1877, it was to his last lecture. He caught cold while making New Year calls and developed severe pyelonephritis. His last note to Mme Raffalovich is undated and the writing is straggling and almost illegible:

> Dear Madame,
>
> Ever since the first of the year I have been overtaken by a frightful attack of abdominal rheumatism. I suffer horribly. I can see absolutely no one. It is as much as I can do to write these few words.

Paul Bert and d'Arsonval, with Mlle Mariette, seem to have assumed charge of the invalid. Paul Bert was expecially distressed by Bernard's solitary state and sent word to his sister in the country. The illness was a very painful one. D'Arsonval wrote to Mme. Raffalovich that Bernard had said: "Nature is sometimes very stupid; what purpose does this pain serve? No purpose either for me or for you. I don't complain of suffering, I only complain that this suffering is of no use to anyone." He was too weak

[1] d'Arsonval, A., *Méd. gén. fr.*, 2: 16, 1935.

even to talk of what he had in mind regarding the experiments on fermentation which had seemed to him to hold such promise. As he lay dying, his thoughts turned to this, his last scientific undertaking, and he murmured, "It would have been nice to have finished it."

He did not wish to see his family, nor did he wish to be disturbed by religious observances. Père Didon, the popular priest who had listened to his lectures, came to see him two days before he died and talked with him for a long time. Bernard spoke of having known suffering, both in body and in mind, in his lifetime. He felt that he had "done what he could." When the subject of his general attitude to religion was brought up, he said that he would have been sorry if his exposition of scientific principles had done anything to disturb or destroy religious faith. He said again, as he had said so often before, that he was unwilling to subscribe to the full doctrine of either positivism or materialism. In this connection he seems to have expressed some dissatisfaction with his *Introduction to the Study of Experimental Medicine.* He spoke of it as "a book of my youth."[1]

The day before his death he was visited by the *curé* of Saint-Séverin, the parish in which he lived. It is not quite clear at what point he began to lose consciousness, and this gave rise later to a bitter controversy as to whether he had in the end accepted of his own free will the last rites of the Church.[2] At the very last, consciousness did return and he now asked for members of his family. We know that Paul Bert

[1] Didon, H., *Rev. de France*, 28: 1–20, 1878.

[2] de Lannessan, J., "Obsèques de Claude Bernard," *Rev. internat. d. sc.*, 1: 255, 1878.

Barral, G., "Correspondance (à propos des obsèques de Claude Bernard)," *Rev. internat. d. sc.*, 1: 381, 1878.

Robin, C., and Pouchet, G., "Claude Bernard," *J. de l'anat. et de la phys.*, 14: 334-8, 1878 (*v.n.*, p. 338).

had sent for his sister and it is probable that one of his daughters was also present before he died.[1] When he began to feel cold and a travelling rug was placed over his feet, he noticed it and said: "This time it will serve me for the voyage from which there is no return, the voyage of eternity." He died on the morning of the 10th of February, 1878.

The following day, M. Bardou, Minister of Public Instruction, announced Bernard's death in the Chamber of Deputies. It was immediately voted that his funeral should be held at the expense of the State, this being the first occasion upon which a scientist was so honoured in France.[2] The ceremony took place on February 16 at the church of Saint-Sulpice, and the body was followed by an impressive *cortège* to the cemetery of Père Lachaise, where Bernard was buried in the same grave as his two infant sons.

[1] Genty, M., *Progr. méd.*, Suppl. 2, 1928. Godart, J., *Almanach du Beaujolais*, p. 41, 1936.

[2] Because this public funeral necessarily took place under the auspices of the Church, great indignation was felt by some of Bernard's disciples and scientific associates. They considered that the Church had unfairly seized the opportunity of making this funeral appear to mark a recantation of Bernard's scientific faith. They protested at being forced, through their respect for the dead, to participate in a ceremony which they considered ridiculous, with its accompaniments of prayer, incense and the sprinkling of holy water. They insisted that if the last rites of the Church had been administered to Bernard, it must have been when he was in a moribund and defenceless condition. Père Didon, on the contrary, stated explicitly that Bernard "died in the faith of his mother and as she who had loved him so much had hoped that he would die." His family were, of course, orthodox Roman Catholics, and on the first anniversary of his death, February 10, 1879, a mass was said for the repose of his soul at the church of Saint-Séverin at the instance of his widow, his daughters, his sister and other relatives.

Bernard would probably have sympathized with the views of his scientific friends without, however, attaching as much importance as they did to the observance of traditional religious rites. Georges Barral has contributed to the controversy a story which makes Bernard's attitude fairly clear. As the two of them were about to cross the rue des Écoles on the day of Bernard's last lecture, they were delayed by the passing of a funeral procession. Bernard pressed the arm of the younger man and indicated with a nod the carriage in which a priest was riding. "When I am taken to the grave," he said, "I hope that I shall not have such a companion." Barral interjected, "But you allowed a Carmelite to attend your course." "Yes, he seemed a nice boy," replied Bernard, "but his presence disturbed me every time I had to draw a philosophical conclusion in my lecture, for I did not wish to upset him."

PART II

CONTRIBUTIONS TO THE SCIENCE OF PHYSIOLOGY

"*On a de la sorte la simple narration de ce qui passe dans l'intérieur du laboratoire et de l'amphithéâtre d'un physiologiste qui travaille et discute la science.*" (i, vii.)

CHAPTER NINE

THE TWO PERIODS OF BERNARD'S CAREER

J'ai même eu le bonheur de trouver dans cette voie des filons inexplorés, qui ont donné à la science des faits imprévus, et soulevé, je crois, des questions nouvelles et fécondes.[1]

ONE OF THE best known physiologists of the last generation, Professor J. N. Langley, in the course of a conversation lamented that, in his estimation, physiology was a finished science. When pressed for a defence of his statement, he said that the functions of the various organs, especially in mammals, had all been set down in outline and all that remained was to fill in the details. In a sense he was right; it will be given to few physiologists of the future to discover entirely new principles. Certainly the time has passed for the duplication of the career of Claude Bernard who laid foundation stones in so many different departments of physiological science.

M. Dastre points out[2] and M. van Tieghem, following his example, emphasizes that Bernard's scientific career falls into two distinct periods, the break occurring at the time of his transference from the Sorbonne to the Museum of Natural History in 1868. At the outset he was, of course, profoundly influenced by Magendie and had adopted as his own Magendie's conception of the function of the chair of medicine at the Collège de France.[3] Therefore, during the earlier period all of Bernard's work was done with an eye to

[1] xi, 467. [2] xvii, v. [3] i, 9.

the practice of medicine, to the curing of the diseases of man; he fought for the introduction of exact scientific methods into the study of the phenomena of life in order that the laws arrived at might be applied directly to man. The great majority of his experiments were performed on mammals or at least on vertebrates. One of the few exceptions is his examination of the liver-like glands of slugs for the presence of sugar.[1] The lectures delivered at the Collège de France, taken as a whole, are the record of this side of his achievement; and here, too, belongs his projected treatise on experimental medicine of which, as we have seen, only the *Introduction* was published as he had originally planned it, although the *Experimental Pathology* and *Operative Physiology* were prepared as substitutes for the missing volumes of the unfinished work.

The second period covers the last decade of his life. During this stage he took a much broader view of the science of life and became in fact, as he had been in theory through his chair at the Sorbonne, a professor of general physiology. When he was about sixty years of age he set himself to learn the facts of plant physiology in order to correlate the principles underlying manifestations of life in plants with those in the animal kingdom with which he was already so familiar. It was for this reason that he read with care in 1874 M. van Tieghem's translation of the famous botanical treatise of Julius Saachs. When he congratulated M. van Tieghem on his effort Bernard said:

> I have read and re-read your Saachs. How many things this book has taught me which I did not know before and which interested me in the highest degree!

[1] i, 101.

> It is an entirely new botany you have revealed to me. If I had known all this some years earlier my researches would have been abridged and I should have turned them in quite another direction. But there is perhaps yet time, and I am setting about it.

Unfortunately there was not time, and instead of the discoveries in general physiology which one might confidently have expected had he lived there remains hardly more than the outline of what he hoped to accomplish. He had planned to publish his lectures on general physiology, given at the Museum, to correspond with those in experimental medicine at the Collège de France, but again he could not carry out his intention. He was able to publish one volume only, and this, the first volume of the *Phenomena of Life Common to Animals and Plants*, was merely the introduction "in the form of an outline" to a treatise on general physiology. M. Dastre, who had been "initiated into (Bernard's) vast designs," made, as we have seen, a second volume of the *Phenomena* out of the remaining lectures. It is striking that Bernard's two most famous books, dealing with the two principal aspects of his activity, should each of them have been merely the introduction to a treatise on a larger scale which he never finished.

It is now more than half a century since Bernard died and sufficient time has elapsed to permit an estimate of the value of his work. Physiologists are all agreed that in four different departments of their science the name of Claude Bernard stands at the head of the list of those who have made lasting contributions. These discoveries all belong to the first, or Collège de France, period and they are: (1) the functions of the pancreas in digestion; (2) the glycogenic function of the liver; (3) the vasomotor system;

(4) the action of certain poisons, notably curare and carbon monoxide. None of these discoveries came as a result of his earliest investigation, but, with the exception of the action of poisons, they were a natural sequel to his initial experimental work.

CHAPTER TEN

EARLY SCIENTIFIC PAPERS

Nous avons souvent répété que la distribution anatomique d'un nerf étant connue, la méthode physiologique expérimentale qu'on emploie pour déterminer ses usages consiste à le couper.[1]

BERNARD BEGAN his experiments with a nerve which innervates a salivary gland. As Macaulay would say, "every schoolboy knows" that digestion starts with the action of saliva in the mouth which changes starch into sugar and modifies the tastes of starchy foods; the food passes to the stomach where gastric juice acts upon it; it then enters the small intestine where it is acted upon by other juices, notably pancreatic juice, and where absorption takes place. Now on each of these topics, and almost in the given order, we find papers from Bernard's hand during the years 1843 to 1850.

His maiden publication appeared in 1843 under the title "Anatomical and Physiological Researches on the Chorda Tympani."[2] Its connection with his studies on digestion is rather accidental because the work was not undertaken on account of the relation (as yet undiscovered) of the chorda tympani to salivary digestion, but because of its relation to the phenomena of facial paralysis. His anatomical deductions in this paper were correct and he helped to establish the fact that the chorda tympani takes its origin from the facial nerve. His physiological deductions were mostly erroneous. Because he failed to see any interference with

[1] v, 244.

[2] "Recherches anatomiques et physiologiques sur la corde du tympan, pour servir à l'histoire de l'hémiplégie faciale," *Ann. méd. psychol.*, 1: 408–39, 1843.

salivary secretion after cutting the chorda tympani, he concluded that this nerve had no influence on the process. Only eight years later Ludwig showed that the chorda tympani contains secretory fibres for the submaxillary salivary gland and he is therefore given the credit for the discovery of secretory nerves.[1]

Bernard's second physiological deduction with regard to the rôle played by the chorda tympani in the sensation of taste is only partly erroneous. He was biased against the possibility that this nerve could be sensory since Magendie had shown that pinching it caused no evidences of pain. The same influence appears in the introduction to his paper when, having spoken rather contemptuously of earlier writings on the subject of the chorda tympani, he says: "Work of this kind is of little profit to physiologists since each author, joining his theory to that of his predecessor, has no merit other than to leave science one more hypothesis." Here, in the words of the prophet, he digged a pit and himself fell into it, for he argued confidently, without verifying his statement by means of his touchstone of experiment, that everyone knows that the lingual nerve is the sensory nerve to the tongue and that, therefore, the chorda tympani cannot serve for taste. Nevertheless, he found that if he cut the chorda tympani on one side and placed dry citric acid on that side of the tongue, there was evidence of taste although the action was diminished and delayed. He therefore advanced the hypothesis that the chorda tympani, while not responsible for taste, must be an accessory to this sense.

[1] Ch. Richet in his *Traité de Physiologie médico-chirurgicale*, p. 225, virtually claims the discovery of the secretory nerves for Dupuy (*J. de méd. de Leroux*, p. 37, 1816) and Bernard (*Mém. Soc. de biol.*, 5 (*C.R.*): 77–107, 1853; cf. xiii, 319) on the ground that the one in 1816 and the other in 1842 had found that after cutting the sympathetic nerve in the neck of the horse, the corresponding side of the head and neck were bathed in sweat.

In the first place, he should have followed the precepts which he afterwards laid down, and tested out what it was supposed that everybody knew; for in this case, as in many another, what everybody knew was not, strictly speaking, the truth. It turns out that it is only those fibres in the lingual nerve which are derived from the chorda tympani itself that are responsible for the sensation of taste; the lingual nerve proper, i.e., the fibres not supplied by the chorda tympani, has to do with other sensations, such as pain, heat, cold, etc. Secondly, he should not have assumed that a dry substance placed on the surface of the tongue will not spread in the adhering layer of mucus to adjoining parts. What happened in his experiments was that the dry test substances became dissolved in the mucus and spread beyond the area to which the nerve had been cut to a normal part of the tongue whose gustatory nerves were intact. Hence, disdainful as he was of mere hypotheses and eager to express his conviction that scientific knowledge can come only through experiment, he failed to practise what he preached and cluttered up the literature with an impossible hypothesis of his own making. He remained unshaken in his belief that the chorda tympani was a motor nerve only, and in his lectures at the Collège de France in 1857 he rejected all evidence that it might be sensory and carry taste fibres.[1]

Another wrong lead, which is more often quoted against Bernard than the foregoing, is the conclusion which he drew from experiments connected with his second paper,[2] namely, that the free acid in gastric juice is not hydrochloric but lactic acid. It has been stated that the acid of gastric juice has probably given

[1] v, 172.
[2] *Du suc gastrique et de son rôle dans la nutrition. Thèse pour le doctorat en médecine*, 34 pp., Paris, 1843.

rise to more discussion than any other subject in physiological chemistry. Prout in 1824[1] found hydrochloric acid in gastric juice, and this was confirmed by the pioneer American physiologist, William Beaumont, in his experiments on Alexis Saint-Martin.[2] Beaumont did not trust his own analyses of gastric juice which had been collected through the fistula of his patient, but sent samples to Professor Dunglison of the University of Virginia and to the famous professor of chemistry at Yale, Dr. Benjamin Silliman. Both these chemists stated that free muriatic (hydrochloric) acid was present in large proportions. Beaumont had also sent a sample to the most famous chemist in the world at that time, Berzelius, but, because of the difficulties of sending the sample from the United States to Sweden in 1833, Berzelius had not replied by the time Beaumont published his book.

Bernard knew the work of Beaumont and considered it so important that he reproduced in his *Experimental Physiology* three pictures of Alexis Saint-Martin's wound taken from Beaumont's book. In spite of Beaumont's findings, however, he concluded, as late as 1856, that the acid of gastric juice was lactic, not hydrochloric, for he had found only the former acid in the stomachs of fasting dogs.[3] We now think that under the conditions of his experiment lactic acid is derived from food which is still present in the stomach. Bernard himself had discovered the presence of food in the stomachs of rabbits even after many days of fasting.[4]

His second argument was that when he distilled a mixture of common salt and lactic acid the same

[1] *Philos. Trans.*, London, Part I, 45–9, 1824.
[2] Beaumont, W., *Experiments on the gastric juice and the physiology of digestion*, 1833.
[3] ii, 394. [4] ii, 389.

stages in the distillate were seen as when he distilled gastric juice. Furthermore, he noted that hydrochloric acid dissolves calcium oxalate while gastric juice does not. We now know that albumins and peptones which are present through partial digestion of proteins prevent the action of the hydrochloric acid in gastric juice. On this occasion, however, Bernard was more cautious than he had been when dealing with the chorda tympani; for, although he again built up a false theory, he added that in advancing this theory that lactic acid was the cause of acidity in gastric juice he did not wish to give the impression that lactic acid was indispensable to digestion; if an acid reaction was all that was necessary, it did not matter what acid was present.[1] He had indeed grasped the truth, and in another experiment he showed that the real digestive agent in gastric juice was indisputably the organic matter whose digestive qualities were destroyed by heating to 85° or 90° C. Destruction by heat is practically the one characteristic common to enzymes; therefore Bernard had really established the fact that digestion in the stomach takes place through the agency of an enzyme and not because of the presence of an acid.

Bernard's third paper[2] has turned out to be even more unfortunate than his first two. He had invented a new method for the study of the spinal accessory nerve. Instead of severing the nerve, as was customary, he pulled it out from the brain through the foramen in the skull through which it emerges. He found that after this operation the animal was voiceless, the vocal cords remaining apart so that breathing was not

[1] *Comp. rend. Acad. d. Sc.*, 19: 1284–8, 1844.
[2] "Recherches expérimentales sur les fonctions du nerf spinal, ou accessoire de Willis étudié spécialement dans ses rapports avec le pneumo-gastrique," *Arch. gén. de méd.*, 4: 397–424; 5: 51–96, 1844.

interfered with. If, however, he severed the recurrent laryngeal nerves which contain fibres from the vagus, the vocal cords nearly closed the glottis so that the animal was in danger of suffocation. He therefore decided that these two nerves have quite different actions on the glottis.

Longet disagreed with Bernard on this point. He concluded from his experiments that the results were the same whichever of the two nerves was cut. Reconciliation of these two opinions was not forthcoming until many years later when other investigators found that in a sense both Bernard and Longet were right. Immediately after cutting the recurrent nerves the glottis closed as Bernard maintained, but a week later the glottis is found to be partially open and remains so, as Longet claimed.

This, however, is not the most disturbing feature of this memoir. We have since found that Bernard's method of tearing the nerve out of the brain was too drastic. It disturbed the neighbouring roots of the vagus; and the vocal cords are not controlled by the spinal accessory, as Bernard thought he had proved, but by the vagus.[1] Oddly enough, it was for this paper that he received the prize in experimental physiology offered by the Academy of Sciences for 1845.

[1] Luciani, L. L., *Human Physiology,* trans. by F. W. Welby, 3: 394.

CHAPTER ELEVEN

PANCREATIC DIGESTION

D'après tous ces travaux, vous voyez, messieurs, que les propriétés du suc pancréatique n'étaient nullement fixées et que ses usages n'étaient pas connus, lorsqu'en 1846 nous fûmes conduit à étudier le rôle du suc pancréatique.[1]

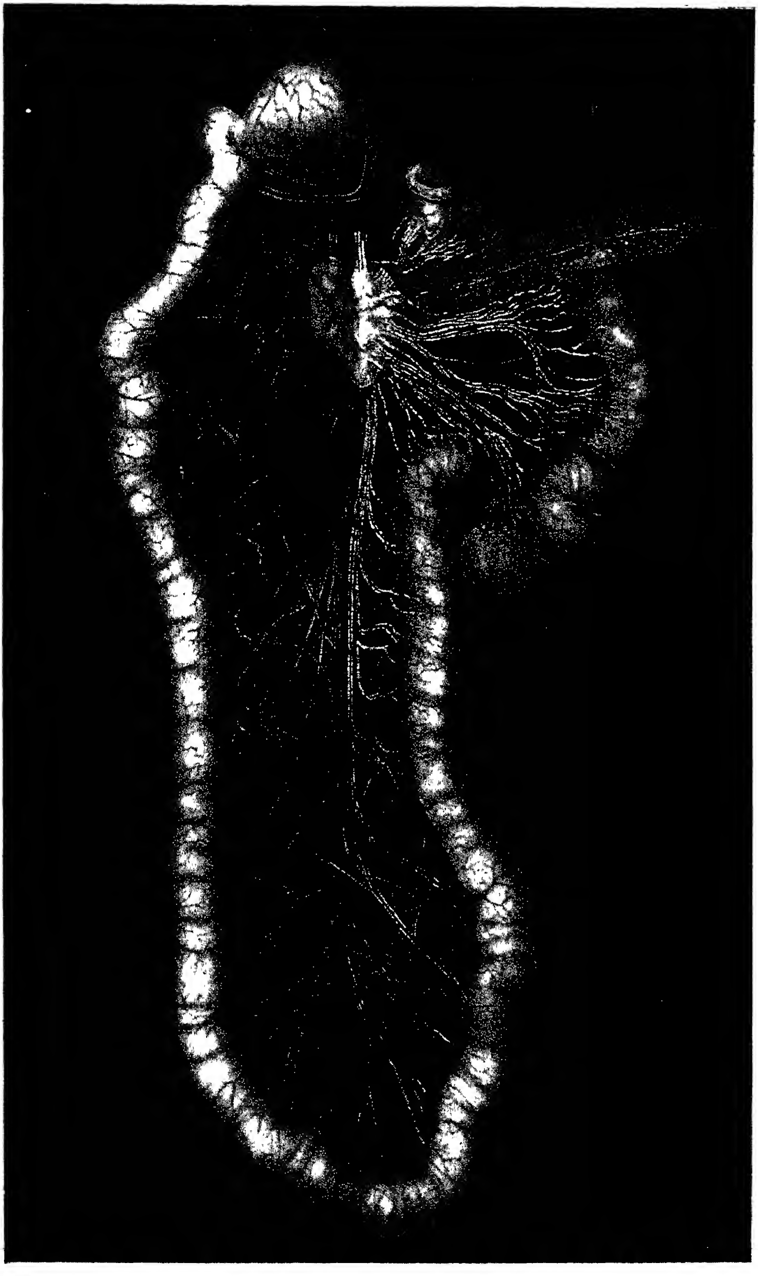
NOT ONE OF the three early papers has stood the test of time, and it was not until Bernard came to investigate pancreatic digestion that he began his really important work. The way in which he happened upon his discovery that pancreatic juice not only acts upon carbohydrates and proteins but also splits neutral fats into fatty acid and glycerine is typical of his experimental method, and was recognized by him as being so when he used it as an illustration in his *Introduction to the Study of Experimental Medicine.*[2] The whole matter started with the chance observation that rabbits brought him from the market had clear urine. This was unusual, since herbivorous animals always have cloudy urine. He solved the mystery; the rabbits had not been fed and were therefore living off their own tissues, having become for the time being carnivores. He now asked himself whether, if he fed a rabbit with meat, its urine would be clear. He found that hungry rabbits would readily eat cooked lean meat and that on such a diet their urine was clear. This result was published in a paper dated March 23, 1846; and it was while autopsying these meat-fed

[1] ii, 178.
[2] viii, 267; cf. vi, 12; vii, 344; *Comp. rend. Acad. d. Sc.*, 22: 534–7, 1846.

rabbits that he noticed that their lacteals were filled with milky-white chyle about 30 to 50 cm. below the pylorus. Now, he remembered that in the dog the white chyle appears in the lacteals very close to the pylorus. What could make this difference? Upon examination he found that in the rabbit the opening of the pancreatic duct into the intestine is about 30 to 50 cm. lower down than in the dog.

His deduction was a flash of genius; pancreatic juice must be responsible for rendering neutral fats absorbable. Here was an entirely new idea. Up to Bernard's time the pancreas had been considered as another salivary gland; in fact, it was called the abdominal salivary gland. The vagueness of physiological teaching on the subject of the pancreas at the time is indicated by the single sentence with which Carpenter in the 1842 edition of his *Human Physiology* disposed of pancreatic digestion: "The chyle is mingled in the duodenum with the biliary and pancreatic secretions, which effect an immediate alteration both in its sensible and chemical properties." Bernard thought that pancreatic juice must be quite different from saliva, since saliva has no effect on neutral fats. By a series of telling experiments he showed that crushed pancreatic tissue mixed with neutral fat and kept at body temperature for a short time caused a splitting of the fat into acid and glycerine.[1] He took pains to point out that as amygdalin and emulsin in bitter almonds are kept apart by partitions within cells and crushing is necessary to bring them together to form prussic acid, so the cells of the pancreas must be ruptured by grinding before they can act upon the fat.

[1] "Du suc pancréatique et de son rôle dans les phénomènes de la digestion," *Mém. Soc. de biol.*, 1: 99–115, 1849.



PANCREAS AND DUODENUM OF A RABBIT DURING DIGESTION OF

A second property which Bernard ascribed to pancreatic juice appeared only when it had been kept for some time without putrefaction having set in; this property was characterized by the appearance of a red colour when chlorine water was added. This colour reaction had already been demonstrated by Tiedemann and Gmelin in 1831,[1] and is now known not to be due to a constituent of pancreatic juice; it is a product of pancreatic digestion. The proteolytic enzyme, trypsin, breaks down the protein present to the constituent amino-acids, among which is tryptophane. It is tryptophane which gives this characteristic colour reaction.

The third property of pancreatic tissue which Bernard described is its ability to transform starch into sugar, and in this respect it does resemble the salivary glands.

Bernard showed that the three characteristics which he had found for crushed pancreatic tissue were to be found in pure pancreatic juice. Regarding the action of the juice on neutral fats he drew an erroneous conclusion.[2] He stated that it acts on fat in two ways: it has a physical action in emulsifying and a chemical one in saponifying. It is now known that the emulsifying action of pancreatic juice depends rather on its alkalinity. If a mixture of liquid fat and water are made slightly alkaline and shaken together, a fine creamy emulsion results.[3] Colloid chemistry with its suspensoids and emulsoids was not even thought of in the 1850's; indeed, it was not until 1861 that Thomas Graham introduced the term *colloid*.

[1] Tiedemann and Gmelin, *Die Verdauung nach Versuchen*, Heidelberg 1831, quoted by E. A. Schäfer, *Textbook of Physiology*, 1: 427.
[2] *Mém. Soc. de biol.*, 1 (*C.R.*): 80, 1849.
[3] Starling, E. H., *Principles of Human Physiology*, p. 39.

Bernard was forced to defend his thesis that pancreatic juice acts on fats from attacks by two members of the Faculty of Medicine of Paris, one of them being the professor of physiology, M. Bérard, who had been on the committee which rejected him in the competition for an assistant professorship in that Faculty, the other M. Longet.[1] M. Bérard claimed that he had found digestion and absorption of fat without the aid of pancreatic juice.[2] His claim was based on finding fat in the chyle of oxen whose pancreatic duct had been ligated. Upon analysing M. Bérard's experiments, Bernard found that no attention had been paid to the fact that in the ox there is more than one pancreatic duct; if only one of them was ligated, instead of not a drop of pancreatic juice reaching the intestine, as M. Bérard claimed, there was a continuous flow of it. This was simply poor experimentation.

M. Longet's criticism, however, was a different matter. Bernard had never forgiven Longet for the part he had played in the story of recurrent sensitivity some twenty years earlier and he saw in this new attack something personal. There were two ways, in Bernard's opinion, in which one might attack a scientific work: attempt to prove that the work was defective (as he had just done with M. Bérard's); or attempt to prove that the work should not be credited to the author. "Usually these two methods of polemic are employed separately, but M. Longet has thought to give his argument greater force by employing them both simultaneously, whence results a new kind of reasoning which is extremely bizarre and difficult to understand." Longet's chief attack consisted in an attempt to show that Eberle had made the discovery before Bernard. This Bernard denied most

[1] vii, 343–70.

[2] *Bull. Acad. de méd.*, 22: 659, 1857.

vehemently; in his opinion Eberle had never even seen pancreatic juice. It is evident that Bernard had no great faith in Eberle's work for, in another place,[1] he made the following comment on Eberle's statement that he had found potassium sulphocyanide in saliva: "It is Eberle himself who gives all these details, and you will be struck at once by their singularity; we need only add that they are pure fantasy; they only prove the influence which imagination can exercise." No wonder that Longet's claim that Bernard's work was a mere repetition of Eberle's acted like a match to tinder. Bernard's defence against Longet's "passionate attack" ended: "It is quite clear that Longet wishes to establish, first, that my experiments signify nothing; secondly, that Eberle had observed the whole thing before. . . . One is led to the incredible conclusion that if the experiments were good, they were Eberle's, if they were bad they were mine!"

Bernard tested the pancreas of the fœtus for its action on neutral fats[2] and he remarked: "It always seemed to me that this characteristic did not appear until a very short time before birth." It was this fact that led Banting in 1921 to try to extract the hormone from the islands of Langerhans in the pancreases of fœtuses of such an age that the acini had not yet developed far enough to produce trypsin. This was the starting-point of the isolation of insulin.

In order to study pancreatic juice in a pure state, Bernard used dogs on which he had operated to produce a pancreatic fistula. He by no means claimed to have been the first person to perform this operation (de Graaf had attempted it two hundred years before) and, in spite of many attempts to make a permanent pancreatic fistula, he had to report both

[1] x, 383. [2] *Mémoire sur le pancréas*, 1856, p. 24.

in his memoir and his *Operative Physiology*[1] that he was not completely successful, although he was able, by using a special silver cannula, to obtain sufficient quantities of pancreatic juice for his experiments. Pavlov later described a method which appears to be more satisfactory.[2]

Bernard noted that there was no secretion from the pancreas of a fasting animal; only when food was given and digestion began did the juice make its appearance; in other words, the passage into the duodenum of acidified chyme was the stimulus for the flow of pancreatic juice.[3] Had he followed this lead, he might have anticipated Bayliss and Starling in the discovery of the first hormone, secretin. Because he found pancreatic juice only after food had been given, he considered that a continuous flow of pancreatic juice would indicate a pathological state. This statement was never seriously questioned until very recently. Using a new technique, which differs from Bernard's or Pavlov's in that the minor pancreatic duct is also ligated so that all communication with the intestine is cut off, Zucker, Newburger and Berg[4] claim that there is a slight continuous flow of pancreatic juice, but that food or secretin greatly increases it temporarily.

Bernard made many attempts to depancreatize dogs, an operation successfully performed by von Mering and Minkowski in 1889, and now performed constantly by those workers in physiological laboratories in different parts of the world who came under the influence of Professor J. J. R. MacLeod during the busy years of the "insulin rush." Had Bernard succeeded in this operation, he would have rendered

[1] *Mémoire sur le pancréas*, p. 43; cf. ii, 215; xv, 581.
[2] Starling, E. H., *Principles of Human Physiology*, p. 529.
[3] ii, 222; vii, 340.
[4] *Am. J. Physiol.*, 102: 193–208, 1932.

his dogs permanently diabetic, and, this great stride once taken, who can say how far his genius would have carried him? Perhaps the terrors of diabetes would have been alleviated some fifty years earlier than they were. He did, however, describe one case of a depancreatized dog.[1] This dog had been operated upon, October 3, 1849, to produce a pancreatic fistula. It later died in a state of profound emaciation, although its appetite was voracious. At autopsy there was found almost complete absence of pancreas, the small bit that remained being quite black. The profound emaciation and voracious appetite would immediately suggest to us diabetes, and the extreme reduction in pancreatic tissue would bear out the correctness of such a diagnosis; but, since Bernard found no sugar in the urine, the possibility of diabetes did not occur to him. The query following his résumé of the experiment, however, is quite significant: "This experiment, which is equivalent to the ablation or, at least, to an alteration of the pancreas, has shown as a symptom severe emaciation accompanied by voracious appetite, finally death by wasting away. Would the suppression of the pancreas have produced the same effect?"[2]

As he was unsuccessful in extirpating the pancreas by surgical means, he tried to put it out of commission by blocking its ducts with solid fats which would not melt at body temperature. While many of these animals died of peritonitis, he did succeed in suppressing the flow of pancreatic juice in others, and he found in these animals, as he had in human patients suffering from pancreatic disturbances, that fat passed out in the fæces undigested.

Although Bernard is credited with discovering that

[1] *Mém. Soc. de biol.*, I (*C.R.*): 204, 1849; cf. vii, 387. [2] vii, 392.

pancreatic juice acts on all three classes of foods: fats, carbohydrates and proteins, he himself thought that his experiments proved its action on the first two only, and not on the third. To be sure, he found "rapid softening and rapid dissolution in certain parts" when proteins, such as lean meat, were subjected to the action of pancreatic juice, but he claimed that this softening soon changed into real putrefaction. His conclusion was that proteins dissolve only on putrefying.[1] He had observed three facts: (1) that pure pancreatic juice would not dissolve proteins completely; (2) that when food leaves the stomach it is first submitted to the action of bile and, somewhat later, to the action of pancreatic juice; (3) that protein, having been acted on by bile, was completely dissolved by pancreatic juice.[2] From these three observations he reasoned that "pancreatic juice cannot act efficaciously on proteins unless bile has acted on them first." The reason why he failed to see complete digestion of proteins is that pure pancreatic juice contains little or no active trypsin; the enzyme is present in an inactive form. To change the inactive form of this proteolytic enzyme to its active form another substance must be present, enterokinase, which is normally to be found in the intestinal juice. It is true, however, that pancreatic juice direct from the duct, without having come in contact with the intestinal mucosa, will digest peptones and will, on standing, even though kept free from bacterial action, gradually become activated.[3] This explains Bernard's results and shows how he came to draw erroneous conclusions.

In admiration of the masterly way in which Bernard

[1] *Mémoire sur le pancréas*, 1856, p. 129. [2] ii, 422.
[3] Starling, E. H., *Principles of Human Physiology*, p. 575.

seized upon a chance observation, and moved step by step towards a much more complete knowledge of the processes of digestion than had been dreamed of, we can forget his relatively minor errors and give him credit for a truly great achievement.

CHAPTER TWELVE

THE GLYCOGENIC FUNCTION OF THE LIVER

J'ai montré qu'il se forme dans les animaux une matière amylacée animal qui n'offre aucune différence avec la matière amylacée végétale.[1]

IT IS GENERALLY considered that the glycogenic function of the liver is the greatest of Bernard's discoveries. In 1843, at the beginning of his scientific career, he undertook to study the fate of the different foodstuffs after their entrance into the body.[2] No one before his time had undertaken this difficult task; all that had been attempted was a statistical account of what went into the organism and what came out. This was "like trying to tell what happens inside a house by watching what goes in by the door and what comes out by the chimney."[3] Since sugar was then the most easily recognizable of the three kinds of foodstuffs, he began with it. He soon learned that there was a great difference in sugars; when he injected cane sugar into the veins of an animal, it reappeared in the urine; when he injected glucose, it disappeared entirely.[4] Bernard asked himself the question: what had become of the glucose? In casting about for an answer, he was at first hampered by the view, then prevalent, that animals were incapable of building up compounds in their bodies and could only break down or destroy by combustion those compounds which had been supplied them from plant material. It was thought that plants

[1] ix, 203. [2] viii, 286. [3] viii, 228.
[4] *V. supra, Du suc gastrique et de son rôle dans la nutrition*, Paris, 1843; cf. xiv, 249.

alone were capable of synthesis; animals could make neither sugars, fats nor proteins.[1] It was the great authority of the famous chemist, Jean-Baptiste Dumas, that gave this theory so much weight in France. Dumas and Boussingault, only shortly before Bernard began his work along this line, had published their well-known essay,[2] the main theme of which was this supposed difference between plants and animals.

Bernard in his lectures on diabetes,[3] gives an account of the arguments which were put forward by the adherents and opponents of this theory during the 1840's. The general idea had originated years before with Lavoisier, but in 1843 the issue was still a live one and Dumas was defending his position with respect to fats. Could the butter in a cow's milk all have come from the grass she had eaten? Dumas said that it could; but a protesting voice from Germany was heard. Liebig claimed that the greater part of the fat contained in the food could be recovered in the excreta. Discussion grew more heated; fattened geese, pigs and even honey bees, in addition to cows, were called in to furnish evidence. Fat in milk naturally led to the question of sugar in milk. Could all the sugar in a bitch's milk have come from the meat she had eaten? In this case Dumas was almost forced to answer, "No"; but at the last moment he was able to save his theory. "This experiment," he said, "must be repeated on account of a peculiar circumstance which throws doubt on the conclusion; the excreta of this dog contained hay; she had chewed her bedding."

Bernard was thoroughly French and the authority of Dumas was powerful. Consequently, he entered upon his work on the assumption that animals were

[1] xiv, 144, 166.
[2] Dumas and Boussingault, *Essai de statique chimique des êtres organisés*, 1841.
[3] xiv, 166.

incapable of making sugar and that, therefore, any sugar present in their bodies must have come exclusively from sugar in their food. His first problem was to discover in what organ sugar so derived was burned. Lavoisier had located the seat of combustion in the lungs.[1] Was sugar burned there?

The first real step towards solving the whole question was taken not by Bernard but by Magendie. The latter had been so interested in the work Bernard and his friend, Barreswill, were doing on the digestion of starch that he chose digestion as the subject for his lectures at the Collège de France for 1846. It was during his experiments for these lectures that he injected starch into the veins of a rabbit and, to his surprise, a few minutes later found not starch but sugar in the blood.[2] In another experiment he found sugar in the blood of a normal dog after it had been fed on starchy food. Heretofore, sugar had been acknowledged to exist in the blood of diabetic patients, but not in healthy persons. Magendie remarked: "This will throw light on the question of diabetes mellitus," and he was right. His contribution, therefore, was the demonstration that sugar was a normal constituent of the blood of healthy animals.[3]

The second step followed in August, 1848, when Bernard and Barreswill showed to the Academy of Sciences alcohol which they had obtained from the fermentation by yeast of sugar from a dog's liver.

[1] vi, 103.

[2] *Comp. rend. Acad. d. Sc.*, 23: 189, 1846.

[3] To Bernard, however, goes the credit for discovering sugar in cerebrospinal fluid. He quoted in one of his lectures in 1855 the case of a patient with a fractured skull who was losing more than a litre of cerebrospinal fluid a day. When a chemist examined this fluid he reported that the patient must be diabetic since sugar had been discovered in the cerebrospinal fluid. Bernard was quite willing to agree that a human being could be rendered diabetic by injury to the base of the brain, as he had rendered rabbits diabetic by puncturing the floor of the fourth ventricle, but it by no means followed that the presence of sugar in cerebrospinal fluid indicated a diabetic condition, since he always found sugar in this fluid in normal individuals. (i. 316.)

Even in animals having no sugar in their diet and nourished on meat exclusively, sugar could be extracted from the liver. The experimenters were forced to conclude that the hypothesis sponsored by Dumas was wrong; the liver of an animal could manufacture sugar. The earlier question of where sugar was burned was abandoned for this more absorbing question, where did sugar in the liver of a meat-fed dog come from?[1]

Piece by piece, the evidence accumulated. Early in 1849, Bernard reported that section of the cerebellar peduncles in rabbits[2] and, a little later, that simply wounding the floor of the fourth ventricle, both in rabbits and dogs, caused the appearance of sugar in the blood and urine.[3] These are the first references to his famous operation of *piqûre* and the production of "artificial diabetes," as he called it. The significance of these experiments was that the nervous system can control the production of sugar in the mammal.

It seems rather a jump from experiments on the chemistry of the liver to experiments involving lesions in the brain, but Bernard's account of how he came upon the fact that puncture of the floor of the fourth ventricle causes hyperglycæmia and glycosuria shows that his mind was working logically.[4] He says at the outset that this discovery was no happy chance. He was following step by step an idea which, he said, in reality was wrong, but which eventually led to the truth. He had long considered the liver to be an organ for secreting sugar; in other words, the liver was really a gland. Now, since the nervous system acts on glands to make them secrete, why should it not act on the liver to make it secrete sugar? He had

[1] *Comp. rend. Acad. d. Sc.*, 27: 249; 253; 514, 1848. Cf. xiv, 160.
[2] *Mém. Soc. de biol.*, 1 (*C.R.*): 14, 1849.
[3] *Soc. Philom.*, p. 49, February, 1849; *Mém. Soc. de biol.*, 1 (*C.R.*): 60, April, 1849.
[4] i, 323.

already found that if he cut the vagi the liver ceased to produce sugar, but he wanted the opposite effect, the production of more sugar; he therefore tried stimulating the vagi, but this did not produce the desired effect. He now remembered that in certain cases, e.g., in his experiments on the salivary glands, he had been able to induce secretion by wounding the appropriate nerve at its origin in the brain. He tried this procedure with the vagi, and found both the animal's blood and its urine charged with sugar. He soon discovered, however, that the vagi were not responsible for this effect, for if he severed them before he performed the operation of *piqûre*, sugar made its appearance just the same; if he left the vagi intact and cut the spinal cord just above the exit of the sympathetic nerve, the production of sugar was interrupted. From this he concluded that a reflex arc was involved, the afferent pathway of which was the vagi and the efferent pathway the sympathetics. The relation of the sympathetic system to the adrenals was, of course, not known at that time, and Bernard wrongly inferred that the sympathetics acted directly on the liver.

In this same year, 1849, he performed a crucial experiment with regard to sugar production in animals. He kept a dog on a diet from which was excluded both sugar and starch. A short time after a meal, when digestion was actively going on, he quickly killed the dog by transecting its spinal cord below the skull. He tied off (1) the mesenteric veins which convey blood from the intestines towards the liver, (2) the veins coming from the pancreas and the spleen and (3) the portal vein a short distance below the liver—in this last vein above the ligature would be found blood which had passed through the liver and flowed

back into the portal vein. Blood was then drawn from all these veins to be analysed for sugar. He found that blood from the upper end of the portal vein alone contained it, i.e. blood entering the liver contained no sugar, but blood leaving (in this case, seeping back from) the liver contained considerable quantities of it. He interpreted this to mean that the sugar must have originated in the liver.[1] Up to this time he had used what he considered to be the only positive test for glucose, namely its fermentation by yeast to alcohol and carbon dioxide.[2] In the memoir read before the Société de Biologie reporting these experiments he told of his use of a new reagent for sugar discovered by Barreswill, the double salt of copper and potassium with tartaric acid. This reagent was very similar to the familiar Fehling's solution still in use in our laboratories.

In October of 1850 appeared the now famous communication "On a new function of the liver" in the proceedings of the Academy of Sciences.[3] Careful examination shows that it is little more than a recapitulation of the work he had published the year before; the only difference was that he had perfected his method of obtaining blood which had passed through the liver. He now took it from the hepatic veins, first tying off the portal vein close to the liver so that there was no back-flow. In order to obtain blood which would normally enter the liver, he drew it directly from the portal vein below the ligature. In this way he had samples of unmixed blood, before and after it had passed through the liver. This was only a matter of technique. There was good reason,

[1] *Mém. Soc. de biol.*, 1: 121–33, 1849.
[2] *Comp. rend. Acad. d. Sc.*, 41: 461, 1855; cf. i. 497.
[3] "Sur une nouvelle fonction du foie chez l'homme et chez les animaux," *Comp. rend. Acad. d. Sc.*, 31: 371–4, 1850.

however, for the presentation of this paper before the Academy of Sciences; it gave it greater prestige to be published in the official records of the Institute of France and there was also the chance of its receiving the prize for experimental physiology—which it did, in 1851. This is why reference is always made to the communication in the *Comptes rendus* of the Academy of Sciences and never to that in the records of the Société de Biologie, although the former was published a year later than the latter.

The question of where the sugar in the blood came from was only partially answered. Bernard was not satisfied with the statement that it came from the liver; he wished to know the details of the process by which the liver could produce sugar. In 1855 he was much impressed by the results of Dr. Lehmann, professor of physiological chemistry at Leipzig, who, while confirming Bernard's work on the relation of sugar to the liver, had found not only that blood leaving the liver contained more sugar than blood entering it, but also that it contained less albumin and no fibrin at all.[1] From this Bernard concluded that "the sugar seems to be produced in the liver at the expense of the albuminoid constituents of the blood." Later on in this same year he referred to this theory without comment either favourable or unfavourable, just as he did to that of Professor Schmidt of Dorpat who thought that sugar appeared in the blood through oxidation of fat.[2] Neither theory was satisfactory to Bernard, for by this time he had finished a new series of experiments and was almost ready to announce a decisive and clear-cut discovery.

He was at this time delivering at the Collège de France the first series of lectures which he was to

[1] *Comp. rend. Acad. d. Sc.*, 40: 744, 1855; cf. i, 471. [2] i, 477.

publish. He had chosen as his subject the work upon which he was then engaged, the glycogenic function of the liver, and in his fourth lecture he coined an expression which has since come to be the technical term for one whole branch of physiology. He said: "The account of the liver shows very clearly that there are *internal secretions*, that is to say, secretions whose products, instead of being poured out to the exterior, are transmitted directly into the blood."[1]

In the following lecture he elaborated the idea. "It must now be clear that there are two functions of the liver in the way of secretion; one an external secretion, producing bile which flows to the outside; the other the internal secretion, forming the sugar which enters immediately into the general circulation."[2] In a communication to the Academy of Sciences later in the year 1855 he used the term *sécrétion intérieure* and stated that to this particular function of the liver he had given the name of *glycogénie* (glycogenesis). He designated the as yet hypothetical substance in the liver which gave rise to sugar as *matière glycogène*, the adjective meaning "sugar-forming." We no longer speak of the glycogenic function of the liver as an internal secretion; it is now considered a special arrangement for storage and liberation of carbohydrate.[3] Later, Bernard pointed out that he included among those glands which furnish internal secretions the thyroid and adrenals, organs which to-day are considered to be perhaps the most typical of all the endocrines or organs of internal secretion.[4]

[1] i, 96. [2] i, 107. [3] Vincent, S., *Secretion*, p. 102.

[4] Some anatomists evidently could not reconcile themselves to giving the name of glands to structures which had no ducts. Bernard referred to these critics rather sarcastically as "backward" anatomists (*anatomists arriérés*) and added: "From the purely anatomical point of view one formerly defined a gland only by its duct. It is clear that we are no longer at that stage; we now understand perfectly that some secretions are poured into the blood, others to the outside." (i, 465.)

By 1855, then, Bernard had to his own satisfaction thoroughly disproved the theory sponsored by Dumas that animals were unable to build up new compounds within their bodies, since he had shown that sugar was produced in the liver of meat-fed animals, and he had already evolved the main points of his theory of the mechanism of glycogenesis. In his lecture of January 30, 1855, he likened the liver to a syringe filled with sugar which injected its contents little by little into the blood; if the rate of injection was too rapid for the physiological needs of the animal at the time, sugar appeared in the urine and symptoms of diabetes were manifest.[1] From this one can see that he was quite conscious of the regulating processes in living organisms which give rise to the steady state of dynamic equilibrium aptly called by Professor W. B. Cannon "homeostasis."

Late in 1855 he announced the results of the new set of experiments which set the capstone on his whole theory.[2] Interestingly enough this new discovery was the result of an accident. Having found sugar in the liver of normal animals, he set out to compare the amounts of sugar to be found in the livers of animals in different conditions of health, nutrition, etc. Like all good chemists he did his analyses in duplicate. One day he was pressed for time, and, after making one analysis immediately following the animal's death, he left the second until the next day.

To his surprise, he found much more sugar in his second analysis than in the first. What was wrong? Had he made a mistake in his observation? Certainly not. Was his method faulty? No, he had checked that thoroughly. Did one part of the liver contain more sugar than another? No, all parts were practically

[1] i, 234. [2] *Comp. rend. Acad. d. Sc.*, 41, 461, 1855; cf. viii, 291.

equally rich in sugar. Should he take a mean of the two readings? No, other physiologists might rely upon averages, but he strongly disapproved of such slipshod methods. The only thing to do was to repeat the observation under the original circumstances. He did so and got exactly the same result as before; there was no doubt that the sugar content of the liver had increased on standing. He then attached one end of a hose to a faucet in the laboratory of the Collège de France and the other end to the portal vein of a freshly removed liver. He flushed the liver for forty minutes with a strong stream of water. At the end of that time no sugar was to be found in the perfusate and none in the tissue of the liver. He now let the washed liver stand for twenty-four hours and again tested both the liquor which had seeped out and the tissue itself for sugar; it was present in abundance in both. It must follow, therefore, that some substance in the liver had changed to sugar on standing; this must be the "animal sugar-forming substance" (*substance animale glycogène*) that he was looking for.

It now remained to isolate this substance in a pure form and to determine its chemical and physical properties. At first he thought that it could not be a carbohydrate, since such substances were supposed to be characteristic only of members of the plant kingdom. It might, however, be a glucoside like amygdalin, and in this idea his friend, the chemist Berthelot, concurred; but work along this line brought no results. Then Bernard thought of the "emulsive decoction" which he had made from liver a short time before.[1] He prepared some of this decoction and added alcohol to it. This gave a white precipitate. The white

[1] i, 156; xiv, 301.

precipitate was dried. Now, there had been no sugar in the fresh decoction, but the dried precipitate, on being moistened, gave the test for sugar. Here was the final proof, the actual isolation of a substance from the liver which, although not itself sugar, could give rise to sugar. It is strange that in the paper describing this discovery Bernard did not use the word *glycogène*, although he had done so earlier in this same year.

The first preparations were naturally crude products, but it was only one step forward to prepare the substance in the pure state, and in March, 1857, Bernard was able to give before the Société de Biologie directions for preparing pure glycogen which are practically the same as those which we employ at the present time. He also demonstrated the physical and chemical properties of this new animal starch. Two days later he made the same announcement before the Academy of Sciences.[1]

It was unfortunate for Bernard that Hensen in Germany had the year before independently accomplished the isolation of glycogen.[2] I have discovered no reference to Hensen's work either in Bernard's shorter papers or in his lectures. His work was quite independent: and, because of his long and patient search for glycogen and his frankness in publishing each step as he went along, he should have full credit for his achievement.

Two years later he added one more bit of detail, namely that in embryonic life the placenta forms glycogen before the liver of the fœtus begins to function; thus glycogenesis begins in fœtal life with a special

[1] *Mém. Soc. de biol.*, 9: 1–7, 1857. *Comp. rend. Acad. d. Sc.*, 44: 578–86; 1325–31, 1857.

[2] Hensen, *Verhandl. d. physiol.-med. Gesellsch. zu Wurzburg*, 7: 219, 1856; quoted by by Schäfer in his *Textbook of Physiology*, 1: 397.
Hensen, *Virchows Arch. f. path. Anat.*, 11: 395, 1857.

organ to supplement the liver.[1] The parallel between the function of the placenta in fœtal life and that of the liver after birth was made more complete by his finding sugar in the fluids which bathe the fœtus, i.e. both in that of the amnion and in that of the allantois, just as he had found sugar in blood leaving the liver after birth. He was much struck by this fact and said that the fœtus at this stage was really diabetic since these fluids, which are in essence fœtal urine, contained large quantities of sugar. As the liver began to take on its glycogenic function sugar gradually disappeared from the fœtal fluids until at the moment of birth there was no sugar to be found in amniotic fluid.[2]

In the paper on this new function of the placenta he announced that he was engaged on some histological work on the liver with a view to locating the region where the internal secretion, glycogen, and the external secretion, bile, were respectively formed. He also showed how granules in certain of the liver cells could be distinguished as glycogen by the fact that they gave a wine-red colour with tincture of iodine. In another communication three months later[3] he informs us that in all his chemico-histological work he was assisted by a pupil, Dr. Kuhne (who later became one of the most prominent of German physiologists). In this note he says that he was able to demonstrate the presence of glycogen in all surface epithelial cells, in mucous cells and in many other tissues, but he found none in bones or nerves. This was his final proof of the existence of that carbohydrate whose presence in the liver he had inferred ten years before because he had found sugar in the hepatic

[1] *Comp. rend. Acad. d. Sc.*, 48: 77, 1859.
[2] vii, 406.
[3] *Comp. rend. Acad. d. Sc.*, 48: 673, 1859.

veins of a meat-fed dog. He had brought to a conclusion his part in the story of the discovery of the glycogenic functions of the liver and the isolation of glycogen, a discovery the importance of which for theoretical physiology and for practical medicine can hardly be over-estimated.

Bernard's views were by no means acceptable to all the physiologists of his own time. His own pupil, Pavy, disagreed with him, particularly with regard to the possibility that a normal, healthy liver could produce sugar, going so far as to question Bernard's experimental results upon which the theory was based. Bernard always seemed more annoyed than disturbed by Pavy's criticism and was inclined to meet it with sarcasm. "M. Pavy," he said, would as the result of his opinions, "be led to consider the diabetic patient as a walking corpse, a conception which is certainly bizarre."[1] Another investigator, unnamed, called forth a fierce outburst from Bernard in a lecture given at the Collège de France early in February, 1855.[2] This investigator had the week before published an article to prove "that sugar which one meets with in the animal organism comes exclusively from plants." Bernard had intended to say nothing about the matter, but certain students taking his course asked him to explain the arguments and he felt it his duty to reply to them. We find that the unnamed author was his old opponent, Longet.

Still another investigator, M. Figuier, during the same year, made communications before the Academy of Sciences and published several articles in the *Gazette médicale* to the effect that the blood entering the liver of a meat-fed dog by the portal vein contained sugar if blood were taken from two to four

[1] xiv, 349. [2] i, 268.

hours after a meal. This attack also called forth a violent response from Bernard. Not content with having referred in a lecture to M. Figuier's work, he added an appendix of twenty-eight pages in vindication of his own results to the first volume of his published lectures. "One can," he said, "understand up to a certain point how illusion could creep into [M. Figuier's] reasoning under the influence of certain preconceived ideas, but it is more difficult to understand how one could find sugar in the blood when there was none there; and how one could fail to find it in the blood of the hepatic veins when it was there. The possibility of such contradictions should sadden men who are looking for the truth."[1]

The controversy came up before the Academy of Sciences and Bernard was appointed a member of a committee to examine the work of M. Figuier, but since his own results were questioned he begged to be excused from serving. A committee, consisting of MM. Dumas, Pelouze and Rayer was then appointed. The whole matter resolved itself into the question, does or does not the blood of the portal vein on the way from the intestine to the liver of meat-fed animals contain sugar? The committee's experiments conclusively substantiated Bernard's position: (1) sugar was not present in the blood of the portal vein of a dog fed on cooked meat; (2) the presence of sugar was, on the contrary, easy to establish in the blood collected simultaneously from the hepatic veins of this dog.[2] Bernard, being human, could not refrain from crowing just a bit over this victory. "The committee," he said, "had recognized the error of the results which have been advanced and has re-established the facts

[1] i, 490.
[2] *Comp. rend. Acad. d. Sc.*, 40: 1281, 1855; 41: 461, 1855; cf. i. 510; vii, 97; xiv, 178.

as I had seen them, and so have all those who have repeated them after me."[1]

There is now no physiologist who does not hold to the main points of Bernard's theory, viz. that carbohydrate is stored as glycogen in the liver, is released into the bloodstream from the liver as glucose and is finally burned to carbon dioxide and water.[2] The curious element in the situation is, however, that Bernard was completely unjustified in making these deductions from his original experimental data. The foundation stone of his whole edifice was his finding no sugar in the blood of the portal vein of a meat-fed or a fasting dog, and a great deal of sugar in the blood of the hepatic veins. We have seen that almost as soon as this result was announced it was challenged; and we have also seen how promptly the challenger was silenced by the committee of Bernard's friends in the Academy of Sciences. There is not the slightest doubt that Bernard and the committee, as well as Leconte,[3] Lehmann,[4] Schmidt[5] and others, all obtained the same results. They would naturally have done so, for they all performed their experiments in the same way.

A sentence near the end of the vindicating committee's report throws considerable light upon the situation. It reads:

> Your committee has examined with all the care of which it was capable the products extracted by M. Figuier from the blood of the portal vein of an animal sacrificed under the same conditions (i.e., during digestion of cooked meat) in which the author believed that he recognized the presence of sugar by the aid of Frommherz's reagent.

[1] iii, 449. [2] Starling, E. H., *Principles of Human Physiology*, p. 618.
[3] *Comp. rend. Acad. d. Sc.*, 40: 906, 1855.
[4] *Comp. rend. Acad. d. Sc.*, 40: 589, 1855.
[5] *Comp. rend. Acad. d. Sc.*, 49: 63, 1859.

> Your committee has found none, *employing, it is true, fermentation.*

The italics are mine. The only fair way to have judged the correctness of M. Figuier's results would have been to repeat his work without variation. If the method was faulty, then the method, not the results, should have been condemned.

The question is, did Bernard's methods do what he thought they did? Did they really demonstrate absence of sugar in the portal vein? We must answer flatly, no. M. Figuier was right in his contention that there is sugar in the blood of the portal vein, for there is sugar in all normal circulating blood. Bernard's method was capable of showing the presence of sugar only in concentration above 80–100 mg. per cent., which is the normal fasting level.[1] His attention had been called by Longet to the possibility of an error in method, but he had at once dismissed the suggestion.[2] He never at any time relented on this point. He had no justification for saying more than, "My methods fail to show any sugar in the portal vein"; and he went counter to his own precepts in being so insistent that his results were conclusive. He was quite conscious of the pitfalls set in the way of physiologists as regards method, for he remarked: "Researches on the formation of sugar in animals could be made only after chemistry gave us reagents for recognizing sugar which were much more sensitive than those we had before." He himself made a notable contribution to the accuracy of blood sugar determinations when he showed that sugar gradually disappears if blood is allowed to stand after being drawn.[3] In spite of all this he continued to maintain

[1] Fulton, J., *Canadian M. A. J.*, 27: 427–33, 1932.
[2] i, 457, 497.
[3] xiv, 208; *Comp. rend. Acad. d. Sc.*, 82: 1406, 1876.

his original position in lectures given towards the end of his life: "One sees that the venous blood from the head contains less sugar than blood from other regions; with the exception, however, of blood from the portal vein, which is so poor in sugar that this substance really cannot be estimated quantitatively."[1] In another passage in the same series of lectures he hedged a little: "In dogs fed for a long time on meat, or in fasting dogs, the blood of the hepatic veins is rich in sugar, while that of the portal vein has none, *or much less.*" The italics are mine.

In 1856, another investigator, M. Chauveau, using M. Barreswill's reagent quantitatively, found sugar in both arterial and venous blood, less in the latter than in the former, even after the animal had been fasting for a long time.[2] The implications of this discovery should have disturbed Bernard. The blood in the liver is derived from two sources; some comes from the intestine by way of the portal vein, some comes direct from the general arterial system through the hepatic arteries. If arteries have more sugar than veins, why could not the extra sugar in blood leaving the liver have been brought in by the arterial blood? Perhaps it did not occur to Bernard to consider this possibility, because M. Chauveau went out of his way to state that the well-known work of M. Claude Bernard had shown so conclusively that in animals fed on meat there was more sugar in the blood of the hepatic veins than in that of the portal, that in his own paper he would dispense with the citation of the figures of his analysis on this point. M. Chauveau's work shows that sugar is present in all circulating blood; furthermore, he does not say that the blood of the portal vein contains *no* sugar, but that it contains *less* sugar than

[1] xiv, 216; cf. xiv, 175. [2] *Comp. rend. Acad. d. Sc.*, 42: 1008, 1856.

that in the hepatic veins. Bernard, in 1877, repeated M. Chauveau's observations on venous and arterial blood, using a new quantitative method, and obtained results similar to his; but I cannot find that Bernard applied his new quantitative method to blood from the portal vein; he evidently considered such a procedure superfluous in the light of his earlier experiments.[1] All criticisms aimed at what he regarded as the proof of his theory of the glycogenic function of the liver he declared to be groundless. He had settled the question to his own satisfaction once and for all. So great was his authority that during his lifetime the great majority of physiologists accepted his verdict without demur.

We are now agreed that with the best methods available to-day there may be detected a slight but constant difference in the sugar content of blood entering and leaving the liver, about 110 mg. per cent. entering as against 130 leaving. These amounts and their difference present a striking contrast with the very much larger quantities reported by Bernard, viz., zero for the portal vein and 700 to 9,000 mg. per cent. for the hepatic; but they furnish the experimental proof on which his theory now rests.[2] There is no doubt that the large amounts of sugar which he obtained were due primarily to his method of killing his animals; transection of the spinal cord stimulates all the nerve tracts in it and he had himself shown in his experiments on *piqûre* the profound influence of the nervous system in causing liver glycogen to be changed into glucose. The relatively small differences in the sugar content of blood entering and leaving the liver which we now believe to exist were obtained by

[1] xiv, 274.
[2] Cori, Cori and Goltz, *J. Pharmacol. & Exper. Therap.*, 22: 355, 1923.

drawing the blood from the appropriate vessels in an unanæsthetized, fasting rabbit through a permanent abdominal window, or in a decerebrate preparation.[1] In this way the influence of the nervous system is kept at a minimum.

In his explanation of the action of the nervous system on the production of glucose from glycogen in the liver, Bernard inferred that the sympathetic nerves were largely responsible. This might nowadays lead one to believe that the adrenals which are so intimately bound up with the sympathetic nervous system were entirely responsible for the effects of *piqûre*, and indeed this has been the opinion until recently. Prof. J. J. R. Macleod in his *Carbohydrate Metabolism and Insulin* has a section entitled, "The supposed relationship of the adrenal glands to nervous hyperglycæmia." He concluded at the time of writing (1926) that for nervous hyperglycæmia not only was the presence of the adrenals necessary but the hepatic nerves must also be intact. Not being entirely satisfied with this, he later made a more exhaustive study of *piqûre*[2] and found that decerebration at the level of the pons was a surer means of inducing a rise in blood sugar. The rise did not occur after either operation if the adrenal glands had been removed, provided only small amounts of glycogen were present in the liver and muscles; if there were larger stores of glycogen, decerebration still resulted in a rise in blood sugar but to a less degree than in animals with adrenals intact. One certainly feels that the last word has not yet been said on the subject of the exact mechanism by which puncture of the floor of the fourth ventricle causes sugar to appear in the blood and urine.

[1] Olmsted, J. M. D., and Read, L. S., *Am. J. Physiol.*, 109: 303–6, 1934.

[2] Donhoffer, C., and Macleod, J. J. R., *Proc. Roy. Soc.*, London, B 110: 170, 1932.

Although the study of carbohydrate metabolism began with Bernard almost a century ago, it has by no means ended yet. We are still and shall for many years be engaged in working out the details of these processes. How glycogen is formed in liver and muscles is still a subject of debate. To suggest to certain physiologists that carbohydrates can come from fats is like flaunting a red flag in the face of a bull; others who have attempted to account for the total carbohydrates in the body have been reproached for their frantic search for hypothetical intermediary products; Nobel prize winners, whose work on muscle glycogen had led physiologists to feel that this subject had been settled once and for ever, have changed their minds. We are still being carried forward by the force of the movement which Bernard initiated.

CHAPTER THIRTEEN
VASOMOTOR NERVES

Cet exemple vous montre que l'hypothèse est toujours utile, en ce que, si elle n'est pas confirmée par l'expérience, les faits mêmes qui la démentent deviennent aussitôt le point de départ d'une nouvelle ère de recherches.[1]

THE WHOLE STORY of the discovery of vasomotor nerves did not appear at once; there were two instalments some five years apart. Bernard himself described the manner in which he was led to make the initial experiment which finally resulted in the discovery as an example of an "hypothesis or theory serving as a starting point for experimental research."[2]

For many years physicians had known that paralysis resulting from various nerve lesions was often accompanied by changes in temperature of the parts of the body affected; sometimes the temperature was raised, sometimes it was lowered. Such inconsistencies always challenged Bernard. The principle of determinism was at stake. To him nature was never inconsistent, the conditions must have been different in the cases reported. Here was something to investigate.

The prevailing theory was that animal heat was due to combustion in the blood. Now, of the three types of nerves, sensory, motor and sympathetic, the last named had been shown by anatomists to follow the course of the blood vessels; therefore, Bernard reasoned, in a trunk of mixed nerves paralysis of the sympathetic fibres would cause a slowing down of the chemical reactions and a consequent cooling of the adjacent

[1] xv, 39. [2] viii, 295; xiii, 205.

tissues. He proceeded to cast about for convenient experimental material to test out his hypothesis. In the dog and cat the sympathetic nerve in the neck is almost inextricably bound up with the vagus, but in the rabbit and horse these nerves are quite separate. When he cut the sympathetic nerve in a rabbit he found the exact reverse of what he had been led to expect on the grounds of his hypothesis. Instead of becoming cooler, the side of the head on which the sympathetic nerve had been cut became about 4° to 6° warmer than the other. "Thereupon," he said, "I did as I always do; that is to say, I at once abandoned theories and hypotheses in order to observe and study the fact itself, so as to define the experimental conditions as precisely as possible." These experiments were performed late in the year 1851 and were reported before the Société de Biologie in October.

Bernard was by no means the first to experiment on the sympathetic nerve. In his historical account of earlier work on the functions of this nerve he stated that Pourfour du Petit in 1727 had noticed changes in diameter of the pupil of the eye as a result of cutting the sympathetic in the neck. He reported that he himself had performed this operation many times in 1842, but, like other physiologists, he had failed to observe changes in body temperature, so firmly was his attention fixed on the phenomena occurring in the eye. In his first note he purposely made no attempt to explain why the side of the head on which the nerve had been cut became warmer. He simply asked the question: "Is the increased circulation the cause or effect of increase in animal heat?"

In all subsequent references to his early work on vasomotor nerves Bernard dated the discovery from the note which he read before the Academy of

Sciences, March 29, 1852, "On the influence of the sympathetic nerve on animal heat."[1] Because he found that the turgescence of the blood vessels which was so marked on one side of the head immediately after cutting the sympathetic nerve diminished or even disappeared on the following day, and yet that side of the head remained warmer than the other, he concluded that the elevation of temperature could not be merely an effect of greater activity in the circulatory system.[2] His persistence in linking the sympathetic nerve with the production of animal heat rather than the vasomotor changes somewhat clouds the issue in the first stage of his work. Although he was really dealing with the vasoconstrictor properties of this nerve, the titles of his papers invariably have reference to changes in temperature. The issue became more clear-cut later when he was dealing with active vasodilation and not with passive vasodilation through cutting off of vasoconstrictor influences.

In November of the same year he made a report before the Société de Biologie which contained important observations regarding the action of the sympathetic nerve both on the eye and on the vascular system.[3] He showed that paralysis of the sympathetic nerve in the dog results in the triple response: (1) constriction of the pupil, (2) drooping of the upper eyelid, (3) recession of the orbit. Because these phenomena were described by Horner for man in 1869,

[1] "De l'influence du système nerveux grand sympathique sur la chaleur animale," *Comp. rend. Acad. d. Sc.*, 34: 472–5, 1852.

[2] The explanation of the mechanism by which blood vessels deprived of their nerves become less turgid and eventually acquire, to a certain degree at least, the ability to control their calibre has been a matter of some debate. The present opinion is that the regain of vascular tone following denervation is due to an increased responsiveness of the denervated vessels to various stimuli, including an adrenaline-like substance (source unknown) circulating in the blood stream. (*V.* Grant, R. T., *Heart*, 2: 1, 1935.)

[3] *Mém. Soc. de biol.*, 4 (*C.R.*): 168–70, 1852.

it is known as Horner's syndrome.[1] In addition Bernard stated that stimulation of the upper end of the nerve by means of galvanism had an effect exactly opposite to that of cutting it. While the nerve was being stimulated the skin became pale and the circulation feebler, but when the stimulation was stopped the former condition returned. As it happened, M. Brown-Séquard had published in the Philadelphia *Medical Examiner* for August, 1852, an article containing the following statement:

> If galvanism is applied to the superior portion of the sympathetic after it has been cut in the neck, the vessels of the face and of the ear after a certain time begin to contract; this contraction increases slowly, but at last it is evident that they resume their normal condition, if they are not even smaller. Then the temperature and sensibility diminish in the face and ear and they become on the palsied side the same as on the sound side. When the galvanic current ceases to act, the vessels begin to dilate again, and all the phenomena discovered by Dr. Bernard reappear.

Bernard did not know of Brown-Séquard's experiment when he performed his own. Brown-Séquard was at this time giving lectures on Bernard's recent work and he might have guessed that Bernard's next experiment would be stimulation of the nerve. This was evidently Bernard's attitude, for a year later he wrote:[2]

> On his return to France, M. Brown-Séquard . . . announced that he was the first to see, while in America, that galvanization of the sympathetic leads to cooling of

[1] White, J. C., *The Autonomic Nervous System.* Cf. Fulton, J. F., "Horner and the syndrome of paralysis of the cervical sympathetic," *Arch. Surg.*, 18: 2025–39, 1929.

[2] *Mém. Soc. de biol.*, pp. 77–107, 1853.

the parts and contraction of the arteries. I will not enter into discussions of priority regarding facts which all date from the same year and which developed immediately as natural corollaries of my first experiment.

In the same memoir of November, 1852, Bernard developed at length his reasons for believing that the change in circulation following the cutting of the sympathetic nerve was not the cause of increased heat production but that it was, on the contrary, secondary to it. His principal argument was the same as that which he had advanced before, viz., that on the day following the operation the extreme turgescence of the blood vessels in the rabbit's ear disappeared without causing any marked lowering of the temperature. Bernard evidently assumed that if increased blood-flow was to account for the rise in temperature it must be the increased flow in the larger vessels exclusively. This assumption was not correct, but, if he had only realized it, his own very accurate observations would have furnished him with a clue which might have led to the truth, for presently we come to this statement: "Nevertheless . . . the capillary circulation always remains more visible in the warm ear." He was using the term "capillary circulation" loosely to refer to the smallest visible blood vessels, which are, in fact, arteries and veins, not capillaries. This observation is crucial to the experiment. The feeling of warmth that one gets from the rabbit's ear is not due to the blood rushing through the large vessels, as Bernard seems to have assumed, but to its volume and the rapidity of its flow in the capillaries. Bernard had dismissed the possibility that increased blood-flow in the smaller vessels could be responsible for the higher temperature of the skin because of some experiments which he had done on the fifth cranial

nerve, where cutting the nerve had led to redness of the conjunctiva and a lowering of temperature. It must be stated in Bernard's defence that it is possible to have a red cold skin and also a pale warm skin. In either case, however, it is a matter of blood circulation, the difference being attributable to which of the vessels are distended with blood, arterioles or capillaries.

It was an unwarranted assumption which started Bernard on a wrong track from which he was never to be diverted. For every theory opposing his own he had a ready criticism. Waller in England thought that the effect on the arteries was the same as that on the pupil of the eye, a paralysis of the muscles innervated by the sympathetic nerve so that the arteries were passively relaxed and consequently became dilated and more engorged with blood and therefore warmer. Bernard could not subscribe to the idea that the rise in temperature could be explained by a "supposed" paralysis of the arteries. He used the word "supposed" advisedly because he believed the paralysis to be "a theory rather than a demonstrated fact." If it were simply a matter of paralysis, the enlargement of the artery should, Bernard thought, take place suddenly. He had never so observed it; on the contrary, he had seen the artery contract considerably the instant that the nerve was cut. It is rather astonishing that he should not have seen that this effect was only the result of stimulation of the nerve by cutting it, and that this period of stimulation must pass away before paralysis could manifest itself. He had himself demonstrated that stimulating the nerve electrically caused the blood vessels to contract. Budge in Germany laid stress on the anatomical relations of the sympathetic nerve to the spinal cord,

which in Bernard's opinion added nothing to the argument. Brown-Séquard in America believed with Waller that the arteries were paralysed but held that the resulting stasis of the blood was of importance. Bernard tied off the veins of both ears in a rabbit until there was stasis and found the ear to become colder, not hotter; yet, if he now cut the sympathetic on one side, the corresponding ear grew warmer. Stasis could not account for the rise in temperature.

Twenty-five years later, at the very end of his life, he again stated his position: "I think that it is not possible to explain the heating of the ear (after cutting the sympathetic) simply by a more rapid renewal of blood in its tissue."[1] Although he held tenaciously to his original idea and brought forward more experiments (not very convincing ones) to bolster up his argument, he was eventually forced to yield on the point of paralysis. "When I cut the sympathetic in the neck and the temperature increases in the entire corresponding half of the head, I think that, because the contractile elements of the tissues are relaxed or paralysed, the elementary changes which are the result of chemical reactions increase as well as thermal phenomena."[2] To complete his theory he was forced to suppose that galvanization of the peripheral end of the sympathetic must reduce the hypothetical chemical changes as well as the thermal phenomena. Had he seen clearly that he was dealing only with a vasomotor phenomenon, he would not have become entangled in this web of conflicting theory.

In 1856 Schiff[3] also cut the sympathetic nerve on

[1] xiii, 283. [2] xiii, 291.

[3] *Berner Schriften*, p. 69, 1856; *Untersuch. über Zuckerbildung in der Leber*, Wurzburg, 1859, pp. 153–6. Quoted by Schäfer, E. A., *Textbook of Physiology*, 2: 132.

one side in rabbits. When he excited these animals, or took them for a run in the warmth of the sun, he found that the ear on the uninjured side was the hotter. This he explained on the assumption that there were vasodilator nerves, i.e., nerves which could cause the diameter of blood vessels to increase and so permit more blood to flow through them. Schiff's experiment and the inference drawn from it were unknown to Bernard, so that his forthcoming proof of the existence of vasodilator nerves, which was to supplement his discovery of vasoconstrictor nerves and thus complete our knowledge of the essential points of the vasomotor system, was entirely his own work.

From the very beginning of his experimental investigations Bernard had been interested in the salivary glands and their nerves. In 1857 he reported before the Société de Biologie that the submaxillary gland receives its nerve supply from two sources, not only from the chorda tympani "which accompanies taste fibres in the lingual nerve" (he would have been more accurate had he said "which contains the taste fibres") but also from the sympathetic nerve coming up from the neck.[1] The experiment leading to this observation was performed for the first time in his course at the Collège de France and it is interesting to see how precisely M. Tripier must have recorded the details of this lecture. He describes Bernard's movements in cutting the nerve; how he directs the instrument upwards, then inclines the handle downwards.

> A feeling of paper being torn, accompanied by a sound which you were able to hear, tells us that we have perforated the tympanic membrane. . . . Now I cut the

[1] *Mém. Soc. de biol.*, 9 (*C.R.*): 85, 1857.

> chorda tympani, which provokes cries from the animal, probably because of the sensitivity of the walls of the middle ear. We will now put vinegar on the animal's tongue. If the secretion continues, it will be necessary to look for another pathway for transmission of motor excitation. . . . We now inject the vinegar into the dog's mouth. Here is a drop of saliva which was at the end of the tube; but when this drop is removed, the flow ceases. What we observe here proves, then, that it is the chorda tympani which is the pathway of transmission of impulses to the salivary gland.[1]

This time he did not leave galvanization of the cut end of the nerve until a later date, but demonstrated before the Société de Biologie at the time of making his report that stimulation of the peripheral end of the severed nerve provoked secretion. Here again his experiment was designed to show one particular phenomenon, but all the while another was staring him in the face. This time it was not ten years before the scales fell from his eyes. While experimenting on the elimination of various substances from the kidney he noticed that during renal activity the veins were filled not with the usual dark venous blood but with bright red arterial blood.[2] He wondered whether the situation would be the same in other active organs and the idea came to him to experiment on the salivary glands with their double nerve supply. On December 28, 1857, he found, upon exposing the submaxillary gland with its veins and nerves that the blood flowing through the gland was dark; the gland was not secreting. When he placed a drop of vinegar in the mouth of the experimental animal drops of saliva made their appearance, as in his earlier experiments,

[1] v, 147. [2] viii, 273.

and the dark colour of the blood changed to bright red. He now tried galvanization of the chorda tympani and obtained the same result, making note of the significant fact that the blood flowed more abundantly when it was bright red.

On August 9 of the next year (1858) came the final word on the existence of vasodilator nerves. When Bernard cut the vein of the submaxillary gland in order to estimate the rate of blood flow through it, he found that the drops escaped much faster during stimulation of the chorda tympani than when the gland was at rest; in fact the blood came in pulsations and was as bright red as arterial blood. This, he said, was due to dilatation of the vein which allowed the blood to enter from the artery so fast that it did not lose its pulsatory movement (an example of venous pulse) nor its bright red colour. On the contrary, when he stimulated the sympathetic nerve the blood flow decreased because of the constriction of the vessels, and even stopped; and the colour of the blood became dark. "In the last analysis we see that the two nerves which modify the colour of the venous blood from black to red are two motor nerves which act first of all in constricting or dilating the blood vessels. The sympathetic is the constrictor nerve for the blood vessels, the tympanico-lingual is their dilator."[1]

Thus, in the end, the existence of the vasomotor nerves was brilliantly demonstrated by Bernard. The neatness of his experiments and the completeness of the proof are much more striking than in the case of his discovery of glycogen. The importance of the discovery of the vasomotor system can hardly be overestimated. There is scarcely a question in any branch

[1] *Comp. rend. Acad. d. Sc.*, 47: 245, 1858.

of physiology which does not ultimately involve a discussion of blood supply, and it is to Bernard that we owe our knowledge of one of the chief mechanisms by which the amount of blood going to an organ is regulated.

CHAPTER FOURTEEN

ACTION OF POISONS

Tous les médicaments sont, en définitive, des poisons.[1]

Bernard groups the studies which he made of the action of the poisons carbon monoxide and curare as examples of the situation in which "the starting point for experimental research is an observation."[2] The experiments with carbon monoxide eventually led him to investigate the properties of the red blood cells and those with curare contributed to our knowledge of the excitability of nerve and muscle.

His work on carbon monoxide began in 1846. He poisoned a dog by making it inhale this gas and after its death he at once performed an autopsy. What caught his attention was the fact that the blood was a bright scarlet in all the vessels, even in the veins. He found the same phenomenon in rabbits, birds and frogs. These observations were repeated in his course of 1853–4 and published in Dr. Atlee's notes. Further investigation was delayed until he gave his course of 1856, his first lectures as Magendie's successor.

When he repeated his experiments he found that the corpuscles were most tenacious of this bright red coloration. In order to compare the effects of certain gases, such as oxygen, carbon monoxide and carbon dioxide, on blood, he introduced a definite quantity of blood into known volumes of these gases. He found that oxygen and carbon dioxide were taken up by the blood, but that apparently carbon monoxide

[1] xi, 72. [2] viii, 276, 279.

was not absorbed since its volume remained unchanged. Upon analysing the gas remaining in this last experiment, he found it to contain a certain proportion of oxygen. This he could explain only on the assumption that the oxygen which was present in the blood had been driven off by the carbon monoxide.

He now submitted one sample of blood shaken with carbon dioxide and another shaken with carbon monoxide to an atmosphere of oxygen. The former was able to take up a considerable amount of oxygen, the latter none at all.[1] He concluded that when the blood comes in contact with carbon monoxide, it can no longer function; the gaseous exchange is paralysed; there is an arrest of all the physiological phenomena accompanying respiration and death results from asphyxiation.[2] He had arrived in a rather roundabout way at the very fundamental fact that the globules, i.e., the red blood cells, are responsible for the respiratory properties of the blood.[3]

In 1867 Bernard stated that he had done an experiment which seemed to him to prove that oxygen in the red blood cells was held in a combined state.[4] Starting with the well-known fact that pyrogallic acid readily absorbs oxygen, he injected this acid into the veins of a dog and found that the blood was not deprived of its oxygen. When, however, the blood came into contact with the air in the lungs, oxygen was absorbed and the pulmonary tissue turned black. In spite of this, the arterial blood was not less bright red in colour or less oxygenated. His conclusion was that the chemical action between pyrogallic acid and oxygen had not taken place within the blood.

[1] iii, 184. [2] iii, 195. [3] iii, 201. [4] ix, 186.

He was evidently quoting from memory, for he directed the reader to consult his lectures on *Liquids of the Organism* without giving the page as he usually did when he referred to a specific experiment. The only reference which I have been able to find which gives approximately these results is in a lecture delivered April 30, 1856, and published in the volume on *Toxic Substances*. If this is the correct reference, Bernard's recollection of this experiment, performed more than ten years before, had rather touched up the results.[1] After passing through the lungs the blood became black and was of the consistency of mud. This was because the corpuscles had all been dissolved through the toxic effects of the pyrogallic acid. Only certain organs retained their normal colour, viz., liver, spleen and kidneys, and these organs when exposed to the air became black also. Because these organs had retained their normal colour until exposed to air Bernard argued that the pyrogallic acid had not acted on blood in vessels which were protected from contact with the air, and concluded from this that oxygen was so firmly bound in the red blood cells that it could not be taken from them by pyrogallic acid.

More convincing than the results obtained by injecting pyrogallic acid directly into the animal was his experiment of shaking up arterial blood with pyrogallic acid out of contact with air. In this experiment the blood retained its colour and became black only when air was allowed to enter. This was certainly a more conclusive indication that the oxygen in the blood is bound and not free.[2]

By 1856, therefore, Bernard had grasped two fundamental facts: first, that the red blood cells were responsible for the respiratory function of the blood;

[1] iii, 222. [2] iii, 223.

and secondly, that oxygen was not carried loosely in solution, but fixed or bound. It must be remembered that the word hæmoglobin had not at this time even been invented. It was not until eight years later that Hoppe-Seyler suggested this term for the red colouring matter of the blood.[1]

Bernard's experiments are seldom referred to in a discussion of the oxygen-carrying power of the blood, probably because of the very prominent part played by Hoppe-Seyler in discovering the properties of hæmoglobin. Bernard lectured on this subject in his course of 1869–70 and paid tribute to Hoppe-Seyler in the following terms: "These phenomena have been studied with great care in Germany by a chemical physiologist, M. Hoppe-Seyler, who has made a very complete examination of the crystalline combinations which hæmoglobin forms with different gases."[2] Hoppe-Seyler had run Bernard a close race for priority in his observation of the effect of carbon monoxide on the colour of blood, for he published his work in 1857.[3] Credit is usually given to the two physiologists jointly.

In 1858 Bernard formally put on record the method he had used for two years for finding the quantity of oxygen in red blood cells, a method which was derived from his experiments on carbon monoxide. The principle upon which the method was based was as follows: since carbon monoxide has a greater affinity for red blood cells than oxygen has, the former turns out the latter quantitatively; the carbon dioxide present can be extracted quantitatively by caustic potash; the oxygen can be extracted quantitatively by pyrogallic acid; and by means of the electric spark

[1] *Virchows Arch. f. path. Anat.*, 29: 233–5, 1864. [2] xii, 417.
[3] *Virchows Arch. f. path. Anat.*, 11: 288, 1857.

the carbon monoxide remaining can be oxidized to carbon dioxide and estimated as such.[1]

Bernard was also interested in the effects of carbon monoxide poisoning on its human victims, whether voluntary or accidental, and this aspect of the question has in recent years come into fresh prominence as a result of the great number of deaths caused by the fumes from motor-car engines left running in closed garages. In Bernard's day would-be suicides had recourse to charcoal fumes and there was no means of resuscitation if sufficient carbon monoxide had been inhaled.[2] The new treatment for such cases by injection of methylene blue[3] has made carbon monoxide a much less dependable means of self-destruction.

The South American arrow poison, curare,[4] seems to have been a more fascinating subject of study to Bernard than any of the other poisons which he investigated, and his accounts of its action are quite dramatic. His attention had been called to this peculiar drug in 1844 by his friend, M. Pelouze, who presented him with some arrows tipped with the poison as well as with some of the gummy curare itself. M. Pelouze was merely passing on a gift which he had himself received from a M. Goudot, who had been for ten years a resident in Brazil and had purchased the articles from the Indians in 1842.[5]

The usual mode of entry of curare into animals was through a wound, and the striking thing about its reaction was the quietness with which the victim died. There were no convulsions, no foaming at the mouth,

[1] *Comp. rend. Acad. d. Sc.*, 47: 393, 1858.   [2] xii, 332.

[3] Brooks, M. M., *Am. J. Physiol.*, 102: 145, 1932.

[4] The spelling of the name of this drug differs from writer to writer, probably because of the attempt to spell phonetically the Indian word. Bernard states that there are at least thirteen different versions, ranging from *urari* and *curari* to *woorara*.

[5] iii, 464.

no cries. The following is a description of a typical curare death as given by Bernard.

> With the aid of a poisoned arrow, I made on the back of a rabbit a prick so painless that the animal did not even stop eating. Two or three minutes later it ceased to eat and withdrew to the corner of the laboratory. It placed its body close to the wall and lowered its ears on its back as if it were sleepy. It then rested perfectly quiet and little by little sank lower and lower; first its legs yielded and its head drooped; finally it fell over on its side completely paralysed. Six minutes after being pricked with the arrow, respiration had quietly ceased. The animal was dead.[1]

Bernard also repeats an account of the death of an Indian poisoned by curare, a story originally told by the traveller, Charles Watterton.

> Two Indians were hunting in the forest. One shot a poisoned arrow at a red monkey in a tree above his head, the direction being nearly perpendicular. The arrow missed the monkey and, returned earthward, it struck the Indian in the arm above the elbow. He was convinced that his end had come. "Never shall I bend bow again," he said to his companion. With these words, he removed the little bamboo box containing the poison which was suspended from his shoulder, and, having put it on the ground with his bow and arrows, he stretched himself beside them, said farewell to his companion and his voice was still for ever.

Watterton's comment on this story was: "It will be a consolation for compassionate souls to know that the victim did not suffer, for the wourali destroyed his life gently."[2]

Bernard tried this poison on a frog in June, 1844. The frog soon became paralysed and was apparently dead. An autopsy being performed immediately, the

[1] xviii, 257. [2] xviii, 261.

heart was found beating and continued to do so; the blood appeared normal; stimulation of nerves, however, provoked no muscular movements either reflexly or when motor nerves were stimulated directly; nevertheless, when the galvanic current was applied directly to the muscles there was vigorous contraction. This observation was the starting point of his investigations.

His first publication on curare was a note before the Société de Biologie in June, 1849.[1] This note shows that he had already carried on extensive investigation on this subject, for he had found out why there was no harmful effect when the drug was administered by mouth in much larger doses than would kill an animal if introduced into a wound. It was not because it was destroyed by the digestive juices, since a wound made with an instrument impregnated with the gastric juice of a dog which had ingested curare quickly caused death, but because it was not absorbed in sufficient quantity to do harm. This explained why game poisoned with curare could be eaten with impunity. He had also tried the poison on many kinds of animals and found that birds succumbed soonest, mammals next and cold-blooded animals last. With M. Pelouze, he made a formal communication before the Academy of Sciences in October, 1850, and this paper, like the first, was principally concerned with the mode of entry of the poison and the resistance to it by mucous membranes.[2]

Bernard thought that he had established the major facts about the action of curare: (1) that it must enter the body through a wound, not by mouth; (2) that it attacked the nerves at the point where they enter the

[1] *Mém. Soc. de biol.*, 1 (*C.R.*): 90, 1849.
[2] *Comp. rend. Acad. d. Sc.*, 31: 533, 1850.

muscles so that movement was impossible, although the muscles themselves seemed not to be affected. One further point needed elucidation. Did curare act on the nervous system as a whole? Was sensation as well as motion lost? The quietness of curare death had led observers to suppose that feeling disappeared with the loss of ability to show feeling. Bernard reported in 1857[1] that five years earlier he had found sensation persisting in curarized animals. He had tied ligatures on suitable blood vessels to prevent the poison from reaching a limb or a certain muscle. When he pinched the skin, the limb or muscle from which the curare-laden blood had been excluded could still respond reflexly, although the sensory nerve involved in the reflex had been subjected to the action of the drug.

What, therefore, he asked, must be the sensations of animals and men dying from curare poisoning? Although their death may seem quiet and peaceful and exempt from pain, it must be accompanied "by the most atrocious suffering which the imagination of man can conceive." The intelligence, sensitivity and will are not touched by the poison, but they lose their power over the instruments of movement one after another. Speech disappears first, then movements of the limbs, then those of the face and chest, and finally the movements of the eyes which, as in the case of those dying a natural death, are the last to be extinguished. "Can one conceive of suffering more horrible than that of an intelligent being realizing the gradual loss of all its functions . . . and finding itself, as it were, entombed alive in a dead body?"[2]

Death comes, but what has really caused death in curare poisoning? Life cannot be maintained without

[2] iii, 465. [1] xviii, 296–7.

respiration; the nerve-controlled respiratory muscles can no longer respond once the poison has reached them, and although the heart, which differs from the muscles which move our limbs in having an inherent power of rhythmic contraction, can still beat and send blood around the body, the blood is no longer oxygenated and the animal dies of asphyxiation. This is Bernard's explanation, but as much had already been established in 1815 by Watterton and Brodie. The English observers had injected curare into an ass and it apparently died within a few minutes. They made an incision in the trachea and for two hours regularly inflated the lungs by means of a pair of bellows. At the end of this time the ass lifted its head and looked round. The bellows were stopped and the ass fell back again apparently dead. Artificial respiration was begun once more and was continued for another two hours. The ass finally got up, apparently none the worse for its adventure. The wounds healed readily and the animal became in the course of events "fat and balky."[1]

Bernard's experiments on curare furnished the final proof of the independent excitability of muscle.[2] A history of this question is to be found in several of his lectures.[3] It began with Glisson (1634–77) who first used the term irritability to designate that property by virtue of which living things are capable of responding to an irritating agent by movement involving muscular contraction. Haller (1708–77), a century later, used the term irritability in a slightly narrower sense to mean the power of a living muscle to contract; and having noted that by irritating nerves he caused pain, he used the term sensitivity for the

[1] xviii, 305. [2] Starling, E. H., *Principles of Human Physiology*, p. 126.
[3] x, 64; xvi, 243.

property peculiar to nerves of conveying sensation. Now, is a muscle capable of contracting independently of its nerve? Bernard used curare to destroy communication between nerve and muscle, a communication which is indispensable for voluntary movement, and found that the muscle could respond to stimuli independently of the nerve. He also used curare to show that the heart muscle is similarly independent of nerves, for after administration of curare the heart is incapable of being stopped by vagal stimulation.[1]

The syllogistic form of reasoning which Bernard used to determine the locus of action of curare was long after his time quoted as a model in physiology. Curare does not injure muscle; it does not injure nerve; but it does prevent the muscle from being excited through the nerve; therefore, curare must act at the place where nerve and muscle meet, the myoneural junction. It remained for another French physiologist, M. Lapique, to show that the premises of this syllogism were not entirely correct, for he has found that curare lowers the excitability of muscle while it leaves the excitability of nerve unimpaired. In a curare-poisoned muscle the process of contraction can be set off by relatively vigorous stimulation, such as the application of an electric current directly to the muscle, but the small changes induced by the natural nerve impulse are too feeble to be effective.

Curare has been a useful tool for the physiologist. In proper doses it is a means of quieting animals in order that muscular movement may not interfere with investigation, and it is especially useful when combined with artificial respiration. Not a year goes by but some physiologist makes use of this drug and

[1] iii, 373.

it has even been proposed to employ it in the case of human patients as a sedative.

In the account of his work on poisons which Bernard gave in his *Report on the Progress and Achievements of General Physiology in France* he mentioned two rather spectacular experiments. In the first he injected simultaneously into one of two different veins situated at some distance from each other a salt of the peroxide of iron and into the other yellow potassium prussate, with the result that Prussian blue appeared, not in the blood vessels or in the tissues, but in the stomach and bladder. The reason for this was that to bring about the formation of Prussian blue acid is necessary; the blood is, of course, slightly alkaline, but the stomach secretes hydrochloric acid and urine is acid in carnivores. In the second experiment he brought about the death of an animal by similarly injecting into different veins amygdalin and its ferment emulsin. Neither of these two substances when injected by itself is poisonous but when they come together they evolve hydrocyanic acid which is almost immediately fatal.[1] Bernard used these experiments to establish a difference between living things and the inorganic world.[2] The internal environment in the living organism, because of its alkalinity, does not permit certain chemical combinations to take place in it (e.g., a salt of iron and potassium prussate to form Prussian blue), but it actually provides "the theatre for all those chemical phenomena of fermentation and combustion which accompany vital manifestations of cells" (amygdalin and emulsin being, of course, themselves products of living cells). This might, at first sight, appear to be essentially a vitalistic view, since, according to Pasteur's definition, ferments could come only

[1] iii, 97. [2] xi, 74; ix, 185.

from living cells, but Bernard actually proved nothing more than that those chemical reactions can occur in blood which proceed in a slightly alkaline medium. The very last experiments which he performed were designed to overthrow Pasteur's definition and bring even fermentation into line with the other physico-chemical phenomena of life.

He also showed in a striking way how certain poisonous gases may be eliminated from the body. Hydrogen sulphide when breathed by man and animals is poisonous since it is absorbed by the pulmonary epithelium and enters the arterial blood. If, on the contrary, it is absorbed by way of the intestine, it does no harm, because in this case it passes out through the pulmonary epithelium and leaves the body in the breath. This he proved by placing a piece of paper moistened with a lead salt before the nose of a dog into which hydrogen sulphide had been injected per rectum or through the jugular vein. The paper began within a few seconds to turn black, the characteristic test for this gas.[1]

[1] ix, 85; xi, 439.

CHAPTER FIFTEEN

MISCELLANEOUS DISCOVERIES AND OBSERVATIONS

En physiologie, tout expérimentateur pourra . . . faire (des découvertes imprévues), pourvu qu'il soit bien pénétré de cette idée, que les théories sont tellement défectueuses dans cette science qu'il y a, dans l'état actuel des choses, autant de probabilités pour découvrir des faits qui les renversent, qu'il y en a pour en trouver qui les appuient.[1]

THE MAJOR discoveries by no means exhaust the list of Bernard's permanent contributions to physiology. The records of other valuable observations are sometimes to be found in his contributions to the Société de Biologie or even to the Academy of Sciences, but more often they must be dug out of his lectures. It is naturally not a very satisfactory way of establishing priority to state in a lecture, as Bernard sometimes did, "Ten years ago I discovered such and such a fact . . ." but one must remember that he performed before his classes at the Collège de France many experiments of which the only record is to be found in the published lectures. He did experiments in all branches of physiology and if one turns over the leaves of any comprehensive treatise on this subject one will be struck by the number of times sentences begin with the words, "Bernard was the first to show that . . ."

EXPERIMENTS ON CUTTING NERVES

One of the chief methods of investigation which he took over from Magendie was the observation of the

[1] i, 17.

effect of cutting different nerves in the living animal. It will be remembered that the discovery of glandular nerves dated from Dupuy's observation in 1816 that cutting the sympathetic nerve in the neck of the horse led to constant sweating on that side, so that Schäfer's statement that "Claude Bernard was the first to make observations upon the effect of section of the glandular nerves"[1] is not strictly correct. Bernard repeated Dupuy's experiments and extended the observations to other glands. He found that two or three days after he had cut all the nerves to the submaxillary gland in dogs, i.e., fibres from both the sympathetic and the chorda tympani, saliva began to flow actively from the denervated gland. Secretion continued for five or six weeks, then ceased. These experiments were described in a lecture at the Sorbonne in June, 1864, but were first published in the initial number of a journal edited by Bernard's friend, M. Charles Robin, professor of Histology at the Faculty of Medicine, who had examined these glands for histological changes.[2] Bernard also found continued secretion of the pancreas after removal of the semilunar ganglion.[3] Bernard's term for this phenomenon was *paralytic secretion* because of its resemblance to so-called paralytic reflexes which we now would call inhibitory reflexes and for which Bernard was the first to attempt an explanation.[4] He had noticed that when an animal was strangled by means of a ligature about its neck, the cessation of respiration was not due merely to the shutting off of air, for if an incision were made in the trachea below the ligature so that air could enter and leave the lungs freely, the animal still did not breathe. He considered that the pressure of the ligature on the

[1] Schäfer, E. A., *Text-book of Physiology*, 1: 519.

[2] *J. d'Anat. et Physiol.*, 1: 507, 1864; x, 398.

[3] xv, 594.

[4] Schäfer, *Text-book of Physiology*, 2: 672.

nerves in the neck inhibited respiratory movements. This effect he called a "paralysing reflex."[1] Applying this idea to the submaxillary gland, he said that there are nervous influences from two sources playing on this gland; the sympathetic nerve causes the muscle fibres of its blood vessels to contract, while the chorda tympani causes the vessels to dilate "by paralysing the sympathetic." The chorda tympani acts when the gland is in activity, the sympathetic when it is at rest. Therefore, secretion is a consequence of the paralysing (inhibitory) action of the chorda tympani on the sympathetic.[2] In this explanation he anticipated Sherrington's principle of reciprocal innervation of antagonistic muscles. Bernard was inclined to believe that the inhibiting action took place at a local vasoconstrictor centre within the gland itself. We now hold that the centre for such reciprocal actions lies within the central nervous system. He supposed that this paralytic action was due to the removal of all nervous impulses. Langley, among others, has repeated these experiments on cutting the chorda tympani and has found, as one would expect, that although the fibres of this nerve degenerated within from three to five days, peripheral nerve cells in the salivary gland itself remained intact. It was Langley's idea that these peripheral nerve cells excited the gland to activity after degeneration of the fibres of the chorda tympani. He said: "A final explanation can hardly yet be given, but some observations made on the cat lead me to think that the secretion is the result of nervous stimuli."[3] No one seems to have carried this problem further.

Bernard found some of these "paralytic" effects to

[1] x, 392. [2] x, 398; xiii, 233.
[3] Schäfer, *Text-book of Physiology*, I: 531.

remain practically indefinitely, e.g., increase in temperature after removing the superior cervical ganglion was plainly evident after a year and a half.[1] In this latter case, of course, there could be no regeneration of nervous connection as there might be when the nerve fibres only are severed.

The harmful results following the cutting of certain nerves had led various physiologists to postulate the existence of "trophic" nerves distinct from sensory, motor and sympathetic nerves. These trophic nerves were supposed to preside over the nutrition of the region innervated. The idea had arisen from some early experiments of Magendie[2] where he had cut the fifth nerve inside the skull. Bernard repeated this work and found marked degenerative changes in the eye particularly; the cornea became opaque, the conjunctiva inflamed and in addition ulcers appeared on the lips, etc. He could not subscribe to Longet's hypothesis that the ill effects were due to the ensuing hyperæmia, for he found that these trophic changes were delayed by the extra blood supply and the increase in temperature, nor could he subscribe to the idea of the existence of trophic nerves as distinct and separate from other nerves. He said: "As for me, I think trophic nerves are fundamentally only vasomotor nerves, the vasodilators, as I showed to be the case upon sectioning the fifth cranial pair within the skull."[3] Bernard's point of view has been upheld. In a sense, all nerves are trophic, but there has never been a demonstration of a purely trophic nerve which "directly modifies the nutrition of a tissue independently of inducing vascular changes in it."[4] The trophic lesions following nerve injury are now

[1] v, 492.
[2] *J. de physiol. expér.*, p. 4, 1824.
[3] xiii, 218.
[4] Schäfer, *Text-book of Physiology*, 2: 876.

considered to result from the absence of protective reflexes through loss of the sense of pain.

Bernard held, as do modern physiologists, that secretion of the submaxillary gland is the result of a reflex action. A substance such as vinegar on the tongue gives rise to excitation of the lingual nerve, the impulse is carried to the brain and returns to excite the gland by way of the chorda tympani, which Bernard always referred to as a motor nerve.[1] This is the usual conception of the course of impulses over a reflex arc, an afferent nerve leading towards the central nervous system, an efferent nerve leading away from it. There are, however, experiments which seem to show that under certain conditions the ganglia of the autonomic system which lie outside the central nervous system may serve as nervous centres. Claude Bernard was the first to describe these phenomena.[2] He found that if he cut the lingual nerve together with the chorda tympani above the submaxillary ganglion (this would, of course, sever their connection with the brain) and then stimulated the tongue, not by application of an acid, but by pinching or application of salt or ether, there was secretion of saliva, less copious than when vinegar was placed on the tongue of the intact animal but still unmistakable.[3] The impulse must in this case have passed from the mucous membrane of the tongue to the submaxillary ganglion and thence to the gland. We now prefer not to call this a reflex.[4] It is rather a special case where the effect is the result of stimulation of a recurrent fibre or, perhaps, a dividing branch of a nerve. Such phenomena should be grouped with Langley's "axon-reflex" and with

[1] *J. d'Anat. et physiol.*, 1: 507, 1864; x, 390; motor nerve, x, 345; v, 172.
[2] Luciani, *Human Physiology*, 3: 373 [3] x, 346.
[4] Schäfer, *Text-book of Physiology*, 2: 679.

certain of Bayliss's "antidromic impulses," where stimulation of one branch of a nerve fibre causes an impulse to travel up the first branch to its junction with another branch, then turn back and come out towards the periphery along the other branch.[1] In such an action the central nervous system is not involved.

Bernard not only observed the effects of cutting peripheral nerves but he also performed experiments on the cord and spinal roots. When he cut the dorsal roots going to one hind limb of a frog, he noted that when the frog was held up it used the desensitized limb in freeing itself to a less extent than it did its other limbs and that it leaped with difficulty. He performed the same operation on dogs in 1842 while he was still an *interne* under Magendie and found that they were unable to support their weight on a desensitized limb. He showed that these effects could not be due merely to loss of ordinary cutaneous sensations of pain, touch, etc., for when he denervated the skin of the hind legs of a frog it swam, leapt and moved with its usual skill.[2] These experiments foreshadowed the work of Sir Charles Sherrington on proprioceptive impulses. We now realize that sensory nerve fibres run from the muscles themselves to the spinal cord through the dorsal roots; and the impulses conveyed by these fibres are largely responsible for co-ordination in our movements.

As a result of experiments on transection of the spinal cord at the level of the sixth cranial vertebra Bernard made four important observations: (1) the blood pressure falls;[3] (2) the body temperature falls;[4] (3) the visceral reflexes are increased, e.g., in the

[1] Bayliss, W., *The Vasomotor System*, p. 30. [2] iv, 254.
[3] iv, 381. [4] xiii, 161.

rabbit there is obvious rise in peristaltic activity of the intestines;[1] (4) the flow of urine is suspended because of lowered aortic pressure.[2] It should be noted that the first of these observations led to the discovery of the vasomotor centres in the medulla, the existence of which was to be inferred from Bernard's discovery of vasomotor nerves.

MUSCLE PHYSIOLOGY

Several observations by Bernard foreshadow the intensive work of the last decade in the field of muscle physiology. He found that blood leaving a contracted muscle is more venous than that leaving the same muscle at rest, i.e., although a living muscle always consumes oxygen, the consumption is increased during activity;[3] and there is an accompanying increase in heat production.[4] But if the motor nerve to the muscle is cut, there is very little oxygen consumed, even less than in a normal inactive muscle. This shows that while the constant nerve impulses which keep the muscle in a state of semi-activity which we call tonus cause the muscle to consume a certain amount of oxygen, active contraction causes it to consume still more.[5]

Two other observations may be mentioned in connection with the physiology of muscle. Bernard saw that glycogen in the muscles has a different fate from that which it meets in the liver. Liver glycogen is changed into sugar by means of an enzyme and passes into the blood to be carried to remote tissues. Muscle glycogen undergoes a kind of fermentation which changes it into lactic acid.[6] Ordinarily, when

[1] iv, 381. [2] iv, 381. [3] *Comp. rend. Acad. d. Sc.*, 47: 393, 1858.
[4] xiii, 156. [5] xiii, 148; x, 220. [6] i, 257; xiv, 429.

a muscle dies it stiffens and becomes acid through the change of glycogen into lactic acid, but Bernard made the significant observation that rigor mortis is not necessarily associated with the development of lactic acid. If an animal whose muscles contain no glycogen, i.e., after long fasting, is killed, it passes into rigor mortis and in this case the reaction is alkaline, not acid.[1] According to Professor A. V. Hill this was the first sign of the "revolution in muscle physiology" which broke out on the last day of December, 1929.[2] It had been regarded as proved, once and for all, that the essential chemical reactions taking place during muscle contraction were the conversion of glycogen to lactic acid to furnish an immediate release of energy and the subsequent oxidation of a portion of the lactic acid back into glycogen so that the muscle might be able to contract again. The two compounds, glycogen and lactic acid, contain only carbon, hydrogen and oxygen. For many years it had been claimed in certain quarters that a compound of phosphorus was involved in muscle contraction, but this idea was not generally accepted. The "revolution" began when overwhelmingly convincing evidence was brought forward that a new and unexpected compound of phosphorus was broken down and built up again as a muscle contracts; and the rout of the old guard may be said to have been complete when it was shown that muscular contraction can take place without the formation of lactic acid or even with little or no glycogen present. Contraction and rigor mortis in muscle have many points in common and the significance of Bernard's observation that rigor mortis can occur without the formation of lactic acid now becomes evident.

[1] xiv, 429. [2] *Physiol. Rev.*, 12: 58, 1932.

CLAUDE BERNARD AND HIS PUPILS, ABOUT 1876
Painting by L'hermitte now hanging in the Sorbonne

PLATE VI

Miscellaneous Discoveries and Observations

EXPERIMENTS ON DIGESTION

Bernard's early work on the role of saliva in the process of digestion was reported in his memoir of 1847,[1] and the more important results were subsequently elaborated in his lectures.[2]

Although Ludwig in 1851 discovered secretory fibres for the submaxillary gland in the lingual nerve, he did not make it clear whether these fibres came from the fifth or seventh cranial nerve. When Bernard six years later discussed the question of the particular nerves concerned in salivary secretion he made no mention of Ludwig's work, but he demonstrated to his audience that the chorda tympani was the secretory nerve for the submaxillary gland, so that these fibres must come from the seventh nerve.[3] He also established the origin of the secretory fibres for the parotid gland, which come from the small petrosal nerve.[4] That he did know of Ludwig's work is shown in his reference to the latter's well-known demonstration that salivary secretion is not provoked by mere rise in blood pressure.[5] He found that wounding the floor of the fourth ventricle of the brain caused all the salivary glands to secrete, but if he made the wound a little above the diabetic centre, there was flow from the submaxillary gland only.[6] Stimulation of the central end of the vagus, of the sciatic or of almost any sensory nerve he found to cause a flow of saliva in the unanæsthetized dog,[7] while direct stimulation of the gland itself only resulted in evidences of pain.[8] The saliva obtained from different glands proved to have different compositions; that from the parotid, for example, was

[1] *Arch. gén. de méd.*, 13: 1–29, 1847. [2] ii, vii, xv. [3] v, 148.
[4] v, 158. [5] v, 164. [6] ii, 69, 80; v, 549.
[7] Schäfer, E. A., *Text-book of Physiology*, 1: 492. [8] vii, 263.

much more watery than that from the other glands. Furthermore, the character of saliva from a given gland varied according to the species from which it was taken, i.e., that from the parotid gland of man differed from that from the same gland of the horse, which in turn differed from that from the same gland of the dog or sheep.[1] The quantity of saliva secreted by the different glands was different, the submaxillary secreting twice as much as the parotid and ten times as much as the sublingual. In general, the amount of secretion appeared to be proportional to the weight of the gland.[2] If the food was chewed on one side of the mouth only there was a greater flow of saliva on that side, and different substances introduced into the mouth of a dog provoked salivation to different degrees, vinegar most strongly, sugar not at all.[3] When injected into the blood potassium iodide passed into the saliva; sugar and other substances, such as potassium ferrocyanide, did not appear in the saliva, although these substances could be excreted by the kidney.[4] Saliva itself could be injected into the blood stream without any harmful effects. By means of d'Arsonval's thermocouple-needles he found in 1876 that when he stimulated the chorda tympani the temperature of the submaxillary gland rose; when he stimulated the sympathetic there was a fall in temperature. He saw in this another proof of the existence of "calorific" and "frigorific" nerves.[5] These hypothetical nerves made their appearance in his lectures as a consequence of his contention twenty-five years earlier that it was impossible to account for the rise in temperature of the rabbit's ear which follows cutting the sympathetic nerve on the ground

[1] ii, 44; xv, 500. [2] ii, 82. [3] ii, 82.
[4] i, 303; ii, 99. [5] xiii, 324–7.

of increased blood-flow, an explanation which seems to us to-day wholly adequate to account for the phenomenon.

One of Bernard's often-quoted discoveries in the field of digestion is his location of the glands which secrete hydrochloric acid. The date of the experiment was January, 1850. Into the vein of a fasting rabbit he injected a solution of a soluble salt of iron, the lactate, then followed it with a solution of potassium prussate. In the presence of acid these two salts form Prussian blue, the vivid colour of which is readily detectable in very small quantities. The animal was killed three-quarters of an hour later and an autopsy performed. In the superficial layer of mucus covering the surface of the stomach, especially along the lesser curvature, the blue colour was to be seen. In his comment on this experiment Bernard stated that it not only showed the glands which secrete gastric juice since this product is always acid, but it also showed that the acid did not exist within the glands themselves but that the gastric juice only acquired its acid properties outside the gland when mixed with the other liquids of the stomach.[1] This deduction was correct, and the chief additional fact which has come to light since Bernard's day regarding the secretion of gastric juice is that it does not come from one single kind of gland; the acid comes from one kind of cell and the pepsin from another.

The question of why the stomach of the living animal does not digest itself has perplexed physiologists for many years. It is strange that the inadequate explanation offered by John Hunter more than a century ago, to which Bernard objected, is

[1] vii, 375.

still given to-day, viz., that only dead tissues are digested. Bernard said:

> It is pretended that life puts an obstacle in the way of the chemical reactions which can take place outside the living individual. . . . This is doubtless true, but it is not an explanation. . . . If gastric juice does not digest the walls of the living stomach, it is because during life pepsin cannot be absorbed. The presence of epithelium on mucous surfaces, particularly that of the stomach, completely prevents the absorption of certain organic substances.[1]

In other words, Bernard's theory was one of selective permeability. To illustrate his point he showed his class a dog with a fistula through which the hind legs of a living frog had been introduced into the dog's stomach for about an hour. The frog was still alive and readily moved its front legs. Upon being withdrawn from the dog's stomach the hind legs were found to be partly digested, although still active. He explained this on the ground that the acid of the dog's stomach had deprived the frog's hind legs of their epithelium and, as soon as this protective layer was gone, digestion began. He informed his hearers that one's finger might be left in a dog's stomach for a very long time without its being attacked. This, of course, was an echo of John Hunter's statement that if one holds one's hand in an animal's stomach it will resist digestion, but that if one cuts it off it will not. Bernard's conclusions were: (1) that life is no obstacle to the action of gastric juice; and (2) that different kinds of epithelium vary greatly in their degrees of permeability. We may agree with these conclusions, but they by no means leave us satisfied

[1] ii, 406–7; xvii, 303–4.

that we understand why an organism does not digest its own digestive organs.

In his thesis for the doctorate of medicine in 1843 Bernard reported a discovery which he afterwards referred to as "one of my first physiological discoveries."[1] Cane sugar is a proper food when taken by mouth, but Bernard found that when he injected it into the blood stream it was not assimilated as a food, but it behaved as a foreign substance; even if injected in very small doses it was eliminated unchanged into the urine, in strong contrast to the fate of glucose injected under similar conditions. Later he discovered the reason for this phenomenon. There was in the intestine, and especially in the intestinal contents of animals killed during active digestion, an enzyme which split cane sugar into dextrose and lævulose.[2] Because this enzyme changes a solution of cane sugar from a dextro-rotatory to a lævo-rotatory one, Bernard gave it the name of *ferment inversif* (invertase). Cane sugar, therefore, does not enter the blood from the digestive tract as cane sugar, but as dextrose and lævulose, and both these sugars can be utilized by the organism. With Barreswill Bernard found that gelatine and egg-white behave similarly to cane sugar, since, if injected directly into the blood stream, they too appear in the urine.[3]

In collaboration with Barreswill he experimented on the effects of alcohol on digestion; they found that in moderate doses alcohol excited gastric secretion and in larger doses arrested it. Ether, on the contrary, in fairly large doses, excited gastric secretion.[4]

[1] viii, 286. [2] xiv, 257; xvii, 341.
[3] *Comp. rend. Acad. d. Sc.*, 18: 783, 1844.
[4] iii, 423, 433. It is said that Barreswill was rather a heavy drinker of *eau de vie*, and Bernard, in relating the results of some of these experiments to his friend, told how, after giving dogs various kinds of foods, he had them swallow some brandy. Barreswill interrupted, "I wish you would do the

Although Bernard was greatly interested in bile as the *sécrétion externe* of the liver, he did not add many new facts to our knowledge of this fluid. He was, however, the first to call attention to the thick layer of caseous substance adhering tenaciously to the villi of the duodenum of dogs killed in full digestion which is a precipitate of the products of protein digestion by bile salts.[1] In experiments on the mechanism of the secretion of bile he showed that in the common snail the gland which corresponds to the vertebrate liver has two different secretions; it secretes sugar while digestion is going on and bile during abstinence from food. Bile, therefore, accumulates in the stomach in the intervals between eating, and at the next meal as the food descends the digestive tract it meets this store of bile and digestion begins.[2] The process is similar to that in man except that in man the bile is stored and concentrated between periods of digestive activity in the gall bladder. In dogs Bernard found that touching the sphincter of Oddi, which guards the opening of the bile duct into the intestine, with a glass rod moistened with weak acetic acid caused an immediate flow of bile into the intestine, whereas a weak alkali, such as sodium carbonate, was without effect.[3] These and similar experiments gave rise to the theory that the entry of acid chyme into the duodenum caused a reflex contraction of the gall bladder and relaxation of the

same for me." "Wait," replied Bernard; "after opening the stomachs of these dogs some hours later, I found that there was a noticeable slowing down in the digestion of the food and that many of the different substances had not yet been attacked by the gastric juice. What do you conclude from that?" "I conclude," said Barreswill, with a serious face, "that brandy was not made for dogs."

[1] Luciani, L., *Human Physiology*, 2: 217; cf. xvii, 307.

[2] "*Thèse . . . 1853, pour obtenir le grade de docteur ès sciences naturelles,*" p. 82 ff.; ii,430; xv, 598.

[3] ii, 430.

sphincter. This theory has only recently been abandoned and opinion is divided between the idea of a hormonal and a nervous control of the emptying of the gall bladder.

Although in the earlier period of Bernard's experimental work he performed most of his experiments on mammals, there were certain problems in comparative physiology which claimed his attention, such as the function of the liver-like gland of the snail. Mammals are characterized by the presence of mammary glands for the secretion of milk for the nourishment of their young. In 1786 John Hunter had observed in the crops of adult pigeons of both sexes at the time that their eggs hatched the presence of a secretion which resembled clotted milk.[1] This secretion first appeared in the parents three or four days after the young birds had emerged from the shell. Bernard was also struck by the similarity of this avian secretion to mammalian milk and began an investigation of its properties in the laboratory of M. Rayer, assisted by another friend, M. Davaine.[2] On analysis the secretion was found to contain casein, salts, a fat analogous to butter and a large amount of water; but, unlike mammalian milk it had no sugar. The "crop-milk" of pigeons has recently been re-analysed with an eye to its vitamin content, a feature which, of course, was unknown in Bernard's day, and tribute was paid to his skill and accuracy in analysis, since the results which he obtained over eighty years ago, were in close agreement with figures obtained by modern methods.[3]

[1] Hunter, J., *Observations on Certain Parts of the Animal Œconomy*, p. 191.
[2] vii, 232.
[3] Reed, Mendel and Vickery, *Am. J. Physiol.*, 102: 285, 1932.

Claude Bernard

EXPERIMENTS ON ANIMAL HEAT

Very early in his career Bernard through Magendie had become interested in animal heat. He considered this topic a special legacy to French physiologists from Lavoisier and Laplace, for it was these two French scientists who showed that animal heat is a true combustion, comparable in every way to the fire on the hearth.[1] The idea that physico-chemical changes taking place in living things obey the ordinary laws of physics and chemistry was, in Bernard's opinion, an absolutely fundamental one. He was therefore doubly interested in the question of animal heat, first as a Frenchman and secondly as a general physiologist. It was not until after the war of 1870 that he began to gather together the results of his own experiments and those of others for his course at the Collège de France.

The first significant experiments were devoted to the determination of the temperature of the blood in various parts of the body, and the tables he drew up have found their way into textbooks of physiology.[2] The question Bernard was trying to answer was, what difference, if any, is there in the temperature of arterial and venous blood? This question had arisen in connection with his early ambitious programme to learn the fate of sugar in the body. At first he held to the theory that the sugar was destroyed in traversing the lungs as a consequence of its coming into contact there with oxygen.[3] This was really Lavoisier's old theory that the lungs were the seat of combustion. If this theory were correct, arterial blood which had just left the lungs to start on its way around the body

[1] ix, 4.

[2] xiii, 40; ch. Schäfer, *Text-book of Physiology*, 1: 828.

[3] i, 238.

should have a higher temperature than venous blood returning to the lungs. Without knowing of the contemporary work along these lines which was being done by Liebig and Fick in Germany, Bernard in 1857 returned to the problem which he had begun with Magendie thirteen years before, and with the help of an assistant who was skilful in the use of thermometers he found that blood in the right ventricle of the heart, which came from the body, was always a fraction of a degree higher than blood in the left ventricle, which had just come from the lungs.[1] He also found that blood leaving the liver was warmer than that entering it, and that during digestion blood was warmed on passing through the intestine.[2] All this was incompatible with Lavoisier's theory that combustion, and consequently heat production, takes place in the lungs. Bernard concluded: "This erroneous view had been overthrown and we now say that those chemical phenomena which result in heat production take place towards the periphery of the body, in the very depths of the tissues in contact with the blood."[3] This statement is in perfect agreement with our present ideas of tissue respiration.

In his earlier experiments Bernard had used thermometers, but during the last four years of his life when he repeated these measurements he was particularly fortunate in having as his assistant d'Arsonval who was extraordinarily clever in adapting for use in the physiological laboratory electrical appliances then hardly more than in their infancy. It was with the aid of the thermocouples of d'Arsonval that Bernard's last experiments on the temperatures of the blood were performed and "the study employing these refinements in technique was scarcely

[1] xiii, 45. [2] xiii, 188. [3] xiii, 190.

finished when Claude Bernard was so abruptly removed from science."[1] The thermocouples were composed of a copper and an iron wire soldered together and enclosed in a rubber sound so that the metal was not directly in contact with the tissue.[2]

The earlier experimenters on combustion and respiration, Black, Priestley, Lavoisier and others, placed small animals under bell jars and analysed the enclosed air for changes in oxygen and carbon dioxide. Bernard also used this method, but he realized that the mere presence of excess of carbon dioxide had an effect on respiration. He said: "Animals can live longer in oxygen if we remove the carbon dioxide as fast as it is formed."[3] To avoid the difficulty he constructed a closed circuit through which a pump forced gases; towers of sulphuric acid and calcium chloride removed moisture and potash removed the carbon dioxide.[4] Since these experiments were begun under Magendie they were contemporary with, if not earlier than, those of Regnault and Reiset who are credited with having devised a practical respiration apparatus for animals.[5]

The effect of various drugs on body temperature was investigated by Bernard, particularly that of curare. He had found that after administration of this drug there is a rise in body temperature. Other experimenters, on the contrary, found a lowering of body temperature after curare. Bernard readily explained this discrepancy. He had been concerned with the phenomena occurring while the poison was acting, the others with phenomena arising during the prolongation of the life of the animal by means of artificial respiration. He agreed that under the latter

[1] xv, vii. [2] xiii, 71. [3] iii, 130; x, 46. [4] iii, 115.

[5] Regnault and Reiset, *Ann. de chim.*, 26: 299–519, 1849. Cf. Franklin, K. J., *A Short History of Physiology*, p. 92.

conditions the temperature was lowered, but he claimed that it was not due to curare *per se* but to immobility because of muscular paralysis.[1]

Bernard recorded in his volume of lectures on animal heat many experiments on the effects of high temperature on length of life. This subject first interested him in 1842, but it was not until the appearance of these lectures in 1876 that he published his experiments in detail. He gave as the reason for this delay his desire to repeat them under better conditions.[2] In 1859 he did publish a paper which contained the essence of his results without the detail of the lectures. When a rabbit was placed in an oven at 55–60° C., it succumbed at the end of five or six hours. Immediately after death both the rectal and thoracic temperatures were about 45°. The blood had not been sufficiently altered to cause death, but the heart had passed into rigor and death followed because of its failure to beat. The muscles of the limbs also became rigid and would not respond to galvanization.[3] In the lectures he added that the critical temperature for birds is somewhat higher than that for mammals, 51–2° instead of 45° C., and that hot moist air and immersion in hot water were more quickly fatal than hot dry air.[4]

[1] xiii, 58. [2] xiii, 347.
[3] *Mém. Soc. de biol.*, 11 (*C.R.*): 51–3, 1859. [4] xiii, 350.

CHAPTER SIXTEEN
GENERAL PHYSIOLOGY

Mon but est de montrer que les plantes possèdent comme les animaux, au degré ou à la forme près, la sensibilité, cet attribut essentiel de la vie.[1]

THE ONE NEAT contribution of Bernard to general physiology, aside from showing the universality of starch (glycogen being an animal starch), was the demonstration that plants as well as animals are susceptible to anæsthetics.[2] The theory of the action of anæsthetics prevalent about 1870 was that anæsthesia was merely asphyxia. Bernard criticized this theory adversely and proposed another based on his observation of the action of chloroform on muscle and nerve.[3] Muscle first loses its excitability, then becomes rigid and opaque. Chloroform also causes nerves to become opaque; and if the action has not been pushed too far, the nerve returns to its original state when the chloroform has been eliminated and is again able to conduct impulses. According to Bernard's theory anæsthesia consists in a reversible coagulation of the constituents of the nerve cell.[4] This theory never gained the recognition it deserved, for it has been claimed that in order to produce coagulation a much higher concentration of the anæsthetizing agent is necessary than to produce anæsthesia, and that in addition such coagulation is found to be irreversible. Recently Bernard's theory has been revived and experiments have been performed showing that under the proper conditions

[1] xviii, 218. [2] xvi, 258. [3] xii, 153. [4] xvi, 265.

only a small concentration of anæsthetic is necessary to cause coagulation in certain colloidal systems and even in living yeast cells, and that the process is perfectly reversible.[1] This view is not, however, acceptable to all pharmacologists and in 1932 an article appeared entitled "Claude Bernard's Theory of Narcosis" which completely rejects his hypothesis.[2] Further investigation will be necessary to determine whether the theory will stand or fall.

Of all his discoveries the one in which Bernard seemed to take greatest pride was the demonstration that animals as well as plants produce sugar, and it was this contribution to general physiology which caught the attention of the positivist philosophers of the late nineteenth century. He had found in animals a starch-like substance (glycogen) which, when acted upon by starch-splitting enzymes, gives rise to the same three sugars that plant starch does, and in almost exactly the same proportions.[3] In addition, he had shown that although the formation of sugar from glycogen takes place in the liver, nevertheless the process is under control of the nervous system. In his day the idea was prevalent that the body is merely a bundle of organs, each with its own separate function, but his discovery showed not only that animals as well as plants can manufacture compounds, but that in this process different organs of the animal's body co-operate. The concept of the interrelation and interdependence of organs in the animal body has proved especially fruitful in our day, particularly in our understanding of the action of the ductless glands.

Bernard himself stated his discovery in these terms:

[1] Bancroft and Richter, *Proc. Nat. Acad. Sc.*, 16: 573, 1930.
[2] Henderson and Lucas, *J. Pharmacol. & Exper. Therap.*, 44: 253, 1932.
[3] *Gaz. méd.*, 12: 201–3, 1857.

"I have proved that sugar is produced in the animal by a mechanism identical with that which operates in the plant."[1] This is overstatement. What Bernard had shown was merely that animals are able to derive sugar from the protein of their food, which is not quite the startling discovery which his statement of it implies. He was quite aware that any material in an animal's body must be derived from its food.[2] He was also aware of the rôle of chlorophyll in plants in building up sugar from carbon dioxide and water by means of energy from the sun, and he knew that in the last analysis all animal energy must come from the sun through plants.[3]

Mayer had stated the law of conservation of energy in 1847 and Helmholtz had shortly afterwards laid the foundations of the science of energetics, so that the laws of thermodynamics had been stated and the beginnings of their application to biological phenomena been made in Bernard's own generation. He recognized what Helmholtz was trying to do in attempting to strike a balance between the work obtained from a muscle and the heat of combustion which it developed during contraction,"[4] but his indignation over the Franco–Prussian war made him belittle the German work on metabolism, for example that of Pettenkofer and Voit on energy balance in man. His own contributions to physiology were almost entirely on the qualitative and chemical side and were the complement to those of Helmholtz on the quantitative and physical side. He was content to leave on record as his final conclusion regarding the vital phenomena of plants and animals this statement: "They are in essence identical since the nutrition of plant and animal cells, which are the

[1] xvi, 143. [2] xvi, 143. [3] xvi, 216. [4] xvii, 24.

only essential living parts, cannot have a different mode of existence in the two kingdoms."[1] In his conception of general physiology he placed the emphasis on the resemblance of the metabolic processes in plants and animals, whereas we are now inclined to consider the difference in energy relationships between green plants and all other forms of life to be of greater significance.

[1] xvi, 155.

CHAPTER SEVENTEEN

POSTHUMOUSLY PUBLISHED NOTES ON FERMENTATION

Le rôle de cet organisme (la levure) dans la fermentation fut surtout précisé par M. Pasteur.[1]

After the great public funeral had taken place and Bernard's body had been placed beside those of his two infant sons in the grave at Père Lachaise, his three most faithful disciples, Dastre, Paul Bert and d'Arsonval, decided to go over the papers which the master had left to see if there was anything which should be published. Dastre was already engaged in preparing for publication the lectures at the Museum of Natural History and Duval was to complete the series at the Collège de France, but there was possibly other material which should be presented to the scientific world. D'Arsonval was more familiar with Bernard's domestic arrangements than the others and undertook the search. He found in a drawer in the bedroom at 40, rue des Écoles, notes of experiments on fermentation done at Saint-Julien during the vacation preceding Bernard's death, experiments to which Bernard had referred in one of his letters to Mme Raffalovich. The three young men did not wish to assume responsibility for publishing these notes as they were, so they took them to Berthelot who was an authority on the chemistry of fermentation and a vigorous opponent of Pasteur's theories on this subject. He was emphatic in his approval of the

[1] xvi, 160.

plan to publish these notes and wrote a preface setting forth the circumstances under which they had been found.

It was not until July 20 that the notes appeared in the *Revue scientifique* over Bernard's name with Berthelot's introduction. Evidently the matter had been kept a secret, for their publication came with a shock of surprise to Pasteur. He himself tells how he learned the news.[1] About noon of the twentieth of July, 1878, he was going up the steps of the Academy of Medicine on his way to attend a committee meeting before which he was intending to make one of his detractors apologize when he was greeted by Dr. Moreau, another of Bernard's disciples, who was also bound for the same committee meeting. Moreau waved a copy of the *Revue scientifique* and asked:

"Have you seen this article by Bernard on fermentation?"

"No," replied Pasteur.

"How is it that you did not know about it when Bernard thought so much of you and you were such friends?" exclaimed Moreau. "It is a great scientific event."

As soon as the committee meeting was over and M. Colin had most gracefully eaten his words, Pasteur rushed to his laboratory in order to read the article in his own copy of the journal. He was appalled at what was there. Under the heading "Sundry Notes" he saw an account of various experiments on the fermentation of grapes, dated Saint-Julien, October, 1877. They began:

1. Grape juice, decayed yeast, decayed pancreas, etc.
2. Dry fermented lees of wine.

[1] Pasteur, Louis, *Examen critique d'un écrit posthume de Claude Bernard sur la fermentation*, 1879.

3. Dried grapes swollen in water, the juice does not ferment if the skin is not broken; dried rotted grapes.
4. Add water to rotted grapes.
5. Filter juice of residue of remains of rotted grapes, old rotted grape juice with new juice not rotted will do it.
6. Formation of alcohol independently of cells.

. and so on for several pages.

The sixth statement gave Pasteur the clue to the cryptic phrases. Bernard was looking for the soluble ferment which would change the sugar of grape juice into alcohol without the use of living yeast cells, that hypothetical substance which Pasteur was utterly convinced did not exist and the mere mention of which threw him into a rage. In spite of the dictum of the almost omnipotent Liebig in 1839 that yeast cells could have nothing to do with fermentation, Pasteur was confident that he had shown that they had everything to do with it; there could be no fermentation without living yeast cells. He had also challenged the statement of the celebrated chemist, Gay-Lussac, that there could be no fermentation without air and had shown that this could not be true; in the absence of air there was, to be sure, less growth of fermenting organisms than in the presence of air, but a greater amount of alcohol was formed. This led him to adopt his theory of "life without air," the essence of which was that the living yeast cells, unable to get oxygen from the air, split up the sugar molecule in order to obtain oxygen. Fermentation, therefore, was the result of the attempt of a living cell to get oxygen; and to imagine that fermentation was a simple chemical reaction between sugar and another chemical compound was worse than nonsense. So firmly rooted in Pasteur's mind was the idea that fermentation must be associated with living cells that

to suggest that a soluble ferment might exist meant belief in spontaneous generation, and the effort of his whole scientific life had been directed against this theory.

As he read on through Bernard's notes he came across his own name:

> There must be germs, says Pasteur, since pure air does not give rise to fermentation—this is an experiment to be tried by causing a current of air to be filtered through cotton by the aid of suction. . . . Pasteur does not answer, or makes a poor answer, to Gay-Lussac's fermentation in air obtained from the electric pile. He would say that it contained dormant germs. . . . It must be proved that the formation of alcohol is independent of the presence of any cell. This is the last barrier behind which Pasteur has entrenched himself in his declaration that fermentation is life without air, which is false, since rotting in air engenders alcohol without the cells lacking oxygen.

At the very end of the distinctly fragmentary record of experiments came a section headed,

> Theory of alcoholic fermentation:
> The theory is destroyed.
> 1. There is no life without air, because in air, as in its absence, alcohol forms without yeast;
> 2. The ferment does not come from outside germs, for in aplasmic or sterile juice (from green or rotted grapes) the ferment does not develop, although they contain sugar. If one adds ferment, then they ferment.
> 3. Alcohol is formed by a soluble ferment outside of life.

The last words of the article were: "There are two states to study in fermentation: (*a*) Decomposition; (*b*) Morphological synthesis."

Pasteur could hardly believe that Bernard could have written these notes. They had seen each other

frequently in November and December of 1877; they sat side by side at the meetings of the Academy of Sciences, Bernard on Pasteur's right, and there had been no reference to these experiments nor even any mention of fermentation. Pasteur was deeply hurt, and at the next Monday meeting of the Academy of Sciences expressed his feelings in no uncertain terms:

> From the first line to the last, these notes of Claude Bernard contradict the facts and the conclusions I have often presented before this Academy, and the last twenty lines are absolute condemnation, without any restriction whatsoever, of my views on fermentation in general and on alcoholic fermentation in particular.[1]

Pasteur was inclined to believe that these notes were not a statement of accomplishment, but rather a programme of work, and that this was the reason for Bernard's silence. He found, upon inquiry, that the subject of fermentation had been in Bernard's mind during the months before his death. He had intended to make it the subject of lectures in general physiology at the Museum of Natural History and he had spoken with evident satisfaction to Paul Bert in November and December about his recent work on fermentation, saying that Pasteur's experiments were correct but he saw only one side of the subject. He had also mentioned the matter to Dastre, d'Arsonval and Moreau; and it turned out later that d'Arsonval had been carrying out experiments for him at the Collège de France along the lines of those he had done himself at Saint-Julien. Two or three days before his death, d'Arsonval and Dastre, who were in constant attendance upon him, tried to learn more of

[1] *Comp. rend. Acad. d. Sc.*, 87: 125, 1878.

what he had done and his plans for future work, but Bernard was too ill and could only say: "It is all in my head, but I am too tired to talk."[1]

Pasteur saw such enormous disproportion between the conclusions expressed in these notes and the facts they demonstrated that he could not agree with Berthelot that "the declarations of Claude Bernard, some days before his death, were quite in conformity with the general affirmations of the Saint-Julien notes." He wished to see for himself the original manuscript and with d'Arsonval's aid he compared the original text with that which Berthelot had had published in the *Revue scientifique*. He discovered several changes which he thought gave a more definitive quality to the carelessness of style and outline form which were evident in the original. Then, too, he had received a letter from Dr. Moreau to the effect that the notes were written in the style which Bernard would naturally have used when outlining a series of experiments to be carried out. Bernard often said, "One must try to demolish oneself," meaning that one should hold to a theory only if it could withstand all attacks.

In the margins of the first leaves of the notes were suggestions for his course of lectures at the Museum of Natural History. Much of his writing was illegible, but the last words were: "Then, apropos of nutrition, speak of fermentations, of generation and of innervation." One could only conclude that the question of fermentation had not been so paramount in Bernard's plans for his lectures as Berthelot had intimated. Bernard was not going to give a whole course on fermentation, but meant merely to include the subject among others. In Pasteur's opinion Berthelot

[1] *Comp. rend. Acad. d. Sc.*, 87: 128, 1878.

had given an entirely false impression of the significance of these notes.[1]

In the very last words of the article Pasteur thought he had found a key to the whole situation. Bernard during this vacation was not only performing experiments on fermentation but he was also correcting the last proofs of the first volume of his lectures in general physiology. Pasteur went over this work carefully and, finding Bernard's classification of all living phenomena into acts of organic synthesis or organic destruction, decided that what Bernard had been trying to do was to fit his experimental results into this scheme. To Bernard fermentation came under the head of organic destruction which was really death and this was diametrically opposed to Pasteur's conception, for to him fermentation meant life; there could be no fermentation without life. He suddenly thought how he could prove that Bernard was wrong and at the same time show that his own position was correct. He, like Bernard, owned vineyards near his birthplace, at Arbois in the Jura mountains not far from Saint-Julien, and here by some peculiar circumstance the air-borne yeast cells did not reach the grapes until they were nearly ripe. If he were in time, he could wrap the green grapes in sterile cotton or cover them with glass, so that they would ripen out of contact with yeast. Then he could crush the ripened grapes and see if they would ferment. This was in July. He rushed from Paris to Arbois and found that he was in time; the yeast cells had not arrived. He carried out the procedure which he had planned and waited anxiously for late October when the grapes should be ripe. He was right in his prognostications; the protected grapes when crushed

[1] *Comp. rend. Acad. d. Sc.*, 87: 185, 1878.

would not ferment, while others on the same vine exposed to yeast cells fermented as he expected.

Pasteur was not content. He thought that he should demonstrate the facts before the Academy of Sciences, before those scientists who had heard his defence and Berthelot's further attacks. Enlisting the services of his wife and daughter, he took turns with them in holding the precious vines as they were conveyed to Paris in a special compartment of the express. First the vines were taken to Pasteur's laboratory at the École Normale and on the twenty-fifth of November they were transferred without injury to the Academy of Sciences. "If you crush these grapes in contact with pure air, I defy you to see them ferment," cried Pasteur.[1]

His experiment, of course, was entirely beside the point. He had not really grasped what Bernard was looking for. He had had such a struggle to convince the world that microbes existed, that they caused disease and that they could give rise to such processes as fermentation that he could see nothing but the part played by them as living organisms. He could not dissociate fermentation from life and envisage it as a mere chemical process, so that when he found that there was no fermentation when he prevented living yeast cells from coming into contact with grapes, he thought that he had shown that life was necessary for this process. He felt himself utterly vindicated and expressed regret that Bernard should have been such a slave to his theories that they led him astray; he who had always preached against blind faith in theory had himself fallen into this error. He said:

> In résumé, Bernard's manuscript is a sterile attempt to substitute for well-established facts the deductions of an

[1] *Comp. rend. Acad. d. Sc.*, 87: 814, 1878.

ephemeral system. The glory of our illustrious confrère cannot be diminished by it. The errors of those who have accomplished valiant careers in the sciences have only the philosophic interest which is attached to the knowledge of our human frailty. Men are great only by the services they have rendered, a maxim I am happy to borrow from a page of the last work which Bernard left us before he died.

And then, would it be fair to judge our beloved and regretted master on the ground of mistakes which have come to light in an unsigned article the publication of which he had neither asked nor authorized, but which after his death was found "carefully hidden," as M. Berthelot has informed us.

It is my conviction that if our confrère, M. Berthelot, to whom we owe the appearance of this posthumous article, had not himself been dominated by preconceived ideas, he would not have published, at least in the form he did, the work of the illustrious physiologist.[1]

This was the beginning of a bitter controversy which soon involved other scientists. The issue was sharply defined. On Pasteur's side were ranged those who held that fermentation was a phenomenon which necessarily involved life, while those who ranged themselves with Berthelot and Bernard held equally firmly that fermentation was simply a matter of chemical reaction. This had really been no new attitude on Bernard's part. Had Pasteur attended one of Bernard's lectures at the Collège de France in the course of 1869–70—and he often did attend Bernard's lectures—he would have heard him say:

One must always explain what is obscure by that which is clearer and more definite; it is only falling into an error of the experimental method to explain organic physico-chemical phenomena by a vague and undefined vital

[1] *Comp. rend. Acad. d. Sc.*, 87: 814, 1878.

force. Nevertheless, it is a singular fact that this is a mistake which is made every day by chemists [Pasteur was a chemist] when they attempt to occupy themselves with physiological questions; they always in the end make vital properties intervene. Is it desired, for example, to explain what goes on in alcoholic fermentation? Every one knows that brewer's yeast under certain conditions transforms sugar into alcohol and carbon dioxide and it seems to be sufficient to say that this is the result of the properties of a living organism, yeast. But in these terms the physiological question is not answered: it is necessary to enter into the chemical constitution even of the elementary organism of the yeast cell, to find there the substances which possess the chemical property of being able to dissociate the elements of sugar: it is, in a word, necessary to reduce fermentation to a chemical cause and not to a vital cause, and what proves that this must be the case is that the products of fermentation, alcohol and carbon dioxide, are made by other processes outside life.[1]

Bernard thought that he was on the track of these chemical substances in these experiments with the juice of rotted grapes. We realize now that his attempt to find them had not succeeded, but it was only eighteen years after Bernard's experiments at Saint-Julien and during the very year of Pasteur's death, 1895, that Büchner separated from yeast cells under pressure a substance which when added to sugar fermented it to alcohol and carbon dioxide. The soluble ferment that Pasteur denied so vigorously was found and there was not even a hint of spontaneous generation.

To one who had studied Bernard's character it would seem unthinkable that he would have allowed those rough notes to be printed in the form in which they were found in his note-book. He kept silent

[1] xii, 413.

because he had as yet nothing definite to offer. He was merely feeling his way and it would have been utterly unlike him to make prematurely such positive statements on data which he knew were quite insufficient. One must agree with Pasteur that Berthelot was inconsiderate and that the publication of the notes with so much *éclat* was unfortunate; on the other hand, one is inclined to feel that Bernard came out of the affair, which was none of his asking, with a better standing than Pasteur. From the standpoint of clear, honest thinking and breadth of view Bernard was a greater scientist than Pasteur, although Pasteur outshone Bernard in the spectacular quality of his achievements.

Part III

SPECULATIVE CONTRIBUTIONS

"*L'homme de recherches entraîné à la poursuite d'un problème particulier n'a pas à se préoccuper, autant que dure son effort, du problème général de la science. Ses investigations se concentrent sur un point limité; et pendant qu'il s'occupe à sa tâche dans un coin de l'édifice que la science contemporaine élève avec tant de rapidité, il n'est pas nécessaire qu'il embrasse le plan de cet édifice auquel collaborent tant d'autres études que les siennes. Cependant c'est à réaliser ce plan qu'il travaille d'un manière consciente ou inconsciente, comme maçon ou comme architecte.*" (xvii, 391.)

he took what he described as "Descartes and Leibnitz" down to Saint-Julien to read on country holidays, and that he was always regretting his inability to get on with his philosophical reading. There is still in existence the note-book in which he made a laborious résumé of Victor Cousin's translation of Lennemann's *History of Philosophy* and of Auguste Comte's *Lectures on the Positive Philosophy* for 1830. His appended comment was unappreciative: "There is nothing but dispute and contradiction in all the philosophical contemplations produced by the human spirit." It was probably the dissatisfaction with the general nature of philosophical discourse voiced here rather than the direct influence of Cousin's eclecticism which made him say: "Truth, if one can find it, belongs to all the systems."[1]

He was at all times very emphatic about his dislike for what he called "systems." He refused his patronage impartially to the contemporary advocates of mechanism, materialism and positivism, who scarcely disguised their eagerness to have his authority on their side. The materialists in the end grew peevish, and he was openly attacked in their journal, the *Pensée nouvelle*, for his refusal to take sides, and defended by his pupil, Ch. Richet, in the *Revue scientifique.*[2] The rank and file of the positivists were sure that their creed was the only philosophical framework into which his doctrine of scientific determinism would fit perfectly, although M. Littré was inclined, on reviewing Bernard's whole position, to be more cautious.[3] Bernard himself evidently did not feel sure enough of the

[1] viii, 391.
[2] Correspondence of M. Ch. Richet and M. Letourneau, *Rev. scient.*, 17: 303; 377, 1879.
[3] Duval, M., *Rev. de phil. positive*, 20: 424–44, 1878; Littré, É., *Rev. de phil. positive*, 21: 5–11, 1878.

value of his grumblings in his note-book, e.g. that Comte "creates philosophy out of the generalities of science" and "imagines that he can suppress the moral sentiment of humanity," to elevate them to the dignity of a lecture, for he does not state the grounds of his objections to positivism, beyond its being one of the *systems*, anywhere in his published works.

Materialism and vitalism were another matter. In the reviews which he gave from time to time of the philosophical ideas which had influenced the history of science he maintained an unexpected neutrality. It was not surprising that the champion of determinism should have dismissed the vitalism of the eighteenth and early nineteenth centuries as a sterile doctrine which had done positive harm to the developing science of physiology.[1] It was less obvious why he should have expressed disapproval of the "excesses" of the materialists, while admitting that their attitude had been progressive and fruitful in practice. As it happened, the passage in which he most clearly stated his criticism of the failure of materialism to recognize the unique character of vital processes became the *locus classicus* to which modern vitalists, forgetting or forgiving the anathemas which he hurled at their creed in his lifetime, have confidently referred as a justification for regarding him as in fact a "critical vitalist."[2]

Another controversy in which Bernard seems to have avoided taking sides (and this time a scientific rather than a philosophic one) was that which arose out of the spread of Darwinism as a theory of organic evolution. The opening assault of Darwin and Wallace

[1] xvii, 439.
[2] xvi, 50–51; *v.* Driesch, H., *The History and Theory of Vitalism*, pp. 132–7.

upon mid-Victorian orthodox beliefs in England came with their joint essays in 1858, and this was followed by the publication of Darwin's *Origin of Species by Means of Natural Selection* late in 1859. In Germany Darwinism gained ground rapidly, but in France, perhaps because of the persistent influence of Cuvier, it was more coldly received. Bernard nowhere refers to the doctrine of *transformisme*, as evolution was called in France, nor does he discuss the theories of Darwin or Lamarck. He was acquainted with the reputation of Herbert Spencer, but not, apparently, as the popularizer of evolutionary theory.[1] When he talked of "evolution" in his lectures what he meant was the unfolding of the individual from the germ to the adult.[2] He refers in one passage to the experiments of Geoffrey Sainte-Hilaire,[3] and here he mentions some of the fundamental ideas which always appear in attempts to account for the evolution of species, e.g. variation in types under changing external influences. The doctrine of the transmission of acquired characters is distinctly stated as follows: "The living individual is still capable of acquiring in his lifetime under the influence of cosmic conditions and different modifying agents, various tendencies, normal or pathological, which can then be transmitted by organic tradition, that is to say, heredity." He seemed very sanguine about the artificial production of new species and said: "I think that we should be able to produce by scientific means new organic species, just as we create new mineral species, that is to say, we shall cause to appear organic forms which exist virtually in the laws of organogenesis, but which nature has not yet realized." He had no particular interest in theories regarding the origin of species, or

[1] xvi, 27. [2] xvi, 33, 385–8. [3] ix, 110–13. Cf. viii, 159.

FIRST PAGE OF BERNARD'S SPEECH ON THE OCCASION OF HIS RECEPTION INTO THE FRENCH ACADEMY.

The words "Je ne suis qu'un savant," underlined in pencil, were eventually altered to "C'est l'homme de science que vous avez élu"

PLATE VII

of life itself, unless they led immediately to experimental verification. He said:

> Instead of framing unrealizable hypotheses about the origin of things which lead only to blind and sterile discussion or investigation, the scientist . . . begins with phenomena immediately around him and accessible to his observation; he then climbs from one fact to another until he comes as close as he can to the source of phenomena . . . respecting meanwhile the veil which covers the origin of things from our eyes.[1]

On the philosophic side the real motive behind Bernard's war upon the "invasion and domination of systems"[2] was his fear that philosophic systematization might pave the way to scientific bias. His would-be eclecticism and agnosticism were a by-product of his scientific integrity. He founded his admiration for Descartes, rather oddly, on the belief that his philosophy and his physiology taken together expressed both the spiritual and mechanical points of view,[3] but he thought that he recognized in Vesalius and Harvey a third type of mind, untrammelled by a tendency towards speculation, which was the type most likely by its very freedom from preconceived ideas to attain to scientific truth. He said:

> When a scientist pursues investigation, taking for his starting-point any particular philosophic system, he loses himself in regions too far removed from reality, or else the system gives his mind a misleading assurance and inflexibility which goes ill with the freedom and adaptability which an experimenter should always preserve in his researches. One should therefore painstakingly avoid every kind of system, and the reason which I find for this

[1] ix, 114. [2] viii, 396; xviii, 96.

[3] *V.* photograph of fragment from unpublished manuscript of the *Livre*, *Méd. gén. fr.*, 2: 13, 1935.

is that systems do not exist in nature but only in men's minds.[1]

It was a sort of Nemesis which made his compatriot, Bergson, some sixty years later, seize upon the last clause of this pronouncement to make Bernard appear to be the forerunner of his own philosophical system.[2] There is no doubt that Bernard would have agreed with Bergson about the tendency of logical construction, uncorrected by fresh contacts with reality, to diverge from the natural order, but he would have been alarmed at the over-interpretation of his paradox, "Philosophy should not be systematic."[3]

[1] viii, 386.
[2] Bergson, H., *Nouvelles litt.*, p. 5, November 17, 1928.
[3] viii, 391; xviii, 91.

CHAPTER NINETEEN

THE PRINCIPLE OF SCIENTIFIC DETERMINISM

Enfin la connaissance du déterminisme physico-chimique initial des phénomènes complexes physiologiques ou pathologiques permettra seule au physiologiste d'agir rationnellement sur les phénomènes de la vie et d'étendre sur eux sa puissance d'une manière aussi sûre que le font le physicien et le chimiste pour les phénomènes des corps bruts.[1]

ALTHOUGH ALL THE principal ingredients of his final position may be discovered in scattered passages even in his earliest lectures, it was about 1865 that Bernard first expressed himself deliberately on philosophical questions. The *Introduction to the Study of Experimental Medicine* published in that year was an exposition of the experimental method as applied to physiology and of the principle of scientific determinism upon which it was based, with a metaphysical background very lightly sketched in. Dr. Sigerist has pointed out that it has proved to be one of the few medical books which have not aged quickly.[2] It has been reprinted again and again and copies are still exposed for sale in the booksellers' windows in the neighbourhood of the École de Médecine.

Bernard begins the *Introduction* by establishing the relation between an observational science like astronomy and an experimental science like physiology. An experiment to him is simply an induced or provoked observation. He contrasts the scholastic and experimental approaches to a problem and estimates the

[1] xviii, 73. [2] Sigerist, H. E., *The Great Doctors*, p. 316.

part played by the intellectual processes, intuition and reason, in the experimental method. He says that intuition or feeling begets the experimental idea and reason devises the actual experiment. Once the experiment is started, the experimenter's attitude is one of scientific doubt both as to the soundness of his idea and also as to the value of his means of investigation, but he never entertains a doubt as to the validity of the principle of determinism, a principle which Bernard proceeds to define and elaborate in his next section.

He states that the apparent spontaneity of living beings is no obstacle to experimentation upon them, since the necessary conditions of natural phenomena are absolutely determined in living bodies as well as in inorganic bodies. What he means by the principle of determinism is simply this: under identical conditions the resulting phenomena will be identical, and in opposition to his immediate predecessors he maintained that the principle held for animate as well as inanimate bodies. He introduces here his famous conception of the *milieu intérieur*, the internal environment, showing that phenomena peculiar to living beings, particularly the higher animals, e.g., mammals, take place in a perfected internal organic environment. Determinism implies that the limits of our knowledge are the same for the phenomena of living bodies as for those of inorganic bodies. First causes are in both cases inaccessible and immediate causes in both cases absolutely determined. Experimental science should not trouble itself with the question "why?"; it explains "how," nothing more.

He admits, however, that there are experimental considerations peculiar to living beings. A living being must be treated as a harmonious whole, and a special

technique for experimentation developed. The book concludes with illustrations drawn from his own experiments in which he has applied the principles laid down in the earlier part of the work. These are the bare bones of his argument.

In an age when some notion of modern scientific method is part of the mental furniture of practically the whole literate population there is a certain obviousness about Bernard's laboured distinction between observation and experiment, or his exhortation that the results of an experiment be noted with a mind stripped of hypothesis and preconceived ideas, or his announcement that the imagination comes into play in the devising of experiments. Nevertheless, it is not so easy to devise a perfect experiment as the uninitiated might think. The definition of experiment recently tossed off by Mr. Whitehead sounds as if it had been formulated by a philosopher or a mathematician who had never attempted to force nature to answer a question in the laboratory. He says that "an experiment is nothing else than a mode of cooking the facts for the sake of exemplifying the law."[1] This *ex cathedra* pronouncement has its germ of truth, but Bernard's account of how the mind of a scientist really works deals more adequately with the full situation:

> The true scientist is one whose work includes both experimental theory and experimental practice. (1) He notes a fact; (2) apropos of this fact an idea is born in his mind; (3) in the light of this idea he reasons, devises an experiment, imagines and brings to pass its material conditions; (4) from this experiment new phenomena result which must be observed, and so on and so forth. The mind of a scientist is always placed, as it were,

[1] Whitehead, A. N., *Adventures of Ideas*, p. 111.

> between two observations: one which serves as a starting-point for reasoning, and the other which serves as a conclusion.[1]

It is Bernard's contribution to have described perfectly and out of his own experience the collaboration of mind and nature, of fact and idea, which takes place in the experimental method.

Instead of starting from a fixed and indubitable self-evident truth, as Descartes proposed in his famous method, a method which he never succeeded in putting into practice in his biological investigations,[2] Bernard starts with an hypothesis which is not absolute, nor beyond the reach of experiment. Even when he does verify his hypothesis, this is not the end; there is still doubt. He says:

> The theories which embody our scientific ideas as a whole are, of course, indispensable as representations of science. They should also serve as a basis for new ideas. But as these theories and ideas are by no means immutable truth, one must always be ready to abandon them, to alter them or to exchange them as soon as they cease to represent the truth. In a word, we must alter theory to adapt it to nature, but not nature to adapt it to theory.[3]

In illustration of this point he made the prophetic statement: "The chemist's elements are elements only until there is proof to the contrary."[4] Three-quarters of a century later, physicists and chemists are engrossed in "proof to the contrary."

The fundamental principle which makes experimentation on living things possible is determinism, or, as Bernard puts it in another place,[5] an experiment (on a living being) *perfectly* carried out will always give the same results. To one not familiar with the

[1] viii, 43–4. [2] Stock, H., *The Method of Descartes in the Natural Sciences.* [3] viii, 70. [4] viii, 87. [5] xi, 219.

attitude of scientists in Bernard's youth the amount of space he devotes to this subject and the feeling he displays over it seem quite out of proportion to its importance. His concern is comprehensible when one remembers that leaders like Cuvier, Bichat and even Magendie, the empiricist and Bernard's own master, held that in living things there was a vital force which really acted in opposition to, and could and did nullify, the physico-chemical laws obeyed by inanimate matter. Cuvier's description of a female body in the heyday of youth and health suddenly struck down by death, had made an ineffaceable picture in Bernard's mind. Cuvier said:

> Behold, this rounded and voluptuous form, this gracious suppleness of movement, this gentle warmth, these rose-tinted cheeks, these eyes sparkling with love or the fire of genius, this physiognomy lit up by flashes of wit or animated by the fire of passion; all seem to unite to make an enchanting creature. An instant is enough to destroy this magic spell; often, without apparent cause, movement and feeling cease, the body loses its warmth, the muscles sink away and allow the angular protuberances of bone to appear; the eyes become cloudy, the cheeks and lips livid. These changes are only the prelude to others still more horrifying: the flesh turns blue, then green, then black; it absorbs moisture, and while part of it evaporates in corrupt emanations, the rest liquefies into a putrid mass which soon likewise melts away; in a word, after only a few days, there remains only a little earth and salt; all the other elements have dispersed into the air and water to enter into other combinations.

After these Gallic transports, Cuvier was able to proceed more calmly. He resumed:

> It is clear that this dissolution is the natural effect on the dead body of air, humidity, heat, in short, of all the

> exterior agents, and that it is caused by the particular attraction of these diverse agents for the elements which compose the dead body. Nevertheless, during life this body was surrounded by these very agents; their affinities for its molecules were exactly the same, and they would have been equally successful, had the living body not been held together by a force superior to these affinities, a force which only ceased to act at the moment of death.[1]

Bernard struck with all his might at this conception that there was in living things "a vital force in opposition to physico-chemical forces, dominating all the phenomena of life, subjecting them to entirely separate laws, and making the organism an organized whole which the experimenter may not touch without destroying the quality of life itself"[2]—a force rendering experimentation applicable only to inorganic bodies, not to living ones. Vitalism, he said, implies indeterminism, and he was set like a hair trigger to go off at the very mention of the word. Indeed, about the worst thing he could say of a fellow scientist was that he entertained vitalistic notions. He was exceedingly provoked with his English pupil, Pavy, when the latter claimed that the transformation of glycogen in the liver into sugar did not occur naturally in life but was a morbid process, taking place when the liver was removed from the body. "This," said Bernard, "is harking back to old vitalistic notions."[3] Yet, in another mood he could say: "I should agree with the vitalists if they would simply recognize that living beings exhibit phenomena peculiar to themselves and unknown in inorganic nature."[4]

Probably there is to-day no respectable vitalist who would not agree with Bernard in all he has to say about the reliability of biological material for experimental

[1] xviii, 165–6. [2] xviii, 39. [3] xiv, 347–9. [4] viii, 118.

purposes, and for him this was the essence of the doctrine of scientific determinism. He belonged to a different era from the present one, and the physiologist has no longer to combat the idea of a mysterious and indeterminable vital force. The sponsor of indeterminism in science is now the physicist, who, although he has challenged the exactness of the laws which, in certain circumstances, experimental measurement permits us to lay down for nature, has apparently no wish to destroy our general faith in nature's reliability. There is no question but that Bernard would have been hospitable to any modifications of scientific principle which were grounded in experimental demonstration, but it is perhaps idle to speculate whether his temperament would have inclined him to one side or the other of the controversy arising over the implications for ethics and for metaphysics in the paradoxes of atomic physics. The two circumstances which may have a certain relevancy seem, unfortunately, to bear in opposite directions; on the one hand, Bernard had not abandoned the ordinary and untutored conviction of the freedom of the will, and, on the other hand, he had no confidence in the value of statistics.

He distinguished sharply between his scientific determinism and the philosophic determinism of which Leibnitz was the exponent.[1] He nowhere undertakes to discuss the ultimate, as opposed to the empirical, validity of his determinism. He evidently thought that this principle came under the heading of inaccessible first causes. Philosophic determinism meant to him "the negation of human liberty," whereas scientific determinism, far from being the negation of moral liberty, is, he said, "its necessary

[1] xvi, 56.

condition."[1] The example he gives is this: if the mechanism for moving your arm were not absolutely determined and fixed, you would not be free to move your arm in the direction you wished. It is evident that Bernard believed firmly in the doctrine of free will, but nowhere does he attempt to justify his position.

The discussion of the value of statistics occurs in a passage where Bernard deprecates rashness in the application of quantitative methods to the undeveloped science of physiology. He held, of course, that "the application of mathematics to natural phenomena is the aim of all science, because phenomenal law should always be mathematically expressed"[2] but he thought that mathematical deductions were being made in his day on the basis of situations too crude for the results of calculation to be valuable. He cited (1) an investigation of nutrition based on a balance sheet of all the substances taken into a cat's body and given out during eight days' nourishment and nineteen days' fasting, the kittens which were born on the seventeenth day being calculated as excreta,[3] and (2) an attempted analysis of "average European urine" based on samples from a urinal in a cosmopolitan railway station.[4] His dissatisfaction was with the misuse of a method, not with the method itself.[5] His objection to statistics went deeper. He said: "I

[1] xvi, 61. [2] viii, 227. [3] viii, 232. [4] viii, 236.

[5] Although he criticized the estimation of a phenomenon in kilograms of the animal's body when all sorts of tissues foreign to the phenomenon in question were included, e.g., the calculation of quantity of saliva secreted from body weight taken as a whole (viii, 237), there is no reason to suppose that he would have hesitated to follow the modern procedure of calculating the basal metabolism of man from his height and weight (from which, by the use of the formula of Du Bois, his surface area may be deduced) and his age, and concluding that there was something radically wrong with him if a calculation of his basal metabolism by estimating his oxygen consumption under the proper conditions did not agree fairly closely with that estimated from his surface area.

acknowledge my inability to understand why results taken from statistics are called *laws*; for in my opinion scientific law can be based only on certainty, on absolute determinism, not on probability."[1] This is meant as a criticism of the logic of statistics, which might not be maintained in the face of modern physics and of recent refinements within the science of statistics itself. Bernard's real fear was that statistical methods might serve to bolster up medical empiricism and delay the development of a really scientific medicine. He did not reject the use of statistics but deplored what he thought was in his time a failure to try to get beyond them. He saw no advantage for a practical science like medicine in the establishment of laws which by their very nature gave no information about the particular case.[2]

To the physiologist, the most fascinating part of the *Introduction* is towards the end where Bernard gives concrete examples of how the principles laid down in the earlier parts of the book led him to particular discoveries. One example will suffice:

> In 1857, I undertook a series of experiments on the elimination of substances in the urine, and this time the results of the experiment, unlike the previous examples, did not confirm my previsions or preconceived ideas. I had, therefore made what we habitually call an unsuccessful experiment. But we have already posited the principle that there are no unsuccessful experiments; for, when they do not serve the investigation for which they were devised, we must still profit by observation to find occasion for other experiments.
>
> In investigating how the blood leaving the kidney eliminated substances which I had injected, I chanced to observe that the blood in the renal vein was crimson, while the blood in the neighbouring veins was dark like

[1] viii, 239. [2] viii, 241.

ordinary venous blood. This unexpected peculiarity struck me, and I thus made observation of a fresh fact begotten by the experiment, but foreign to the experimental aim pursued at the moment. I therefore gave up my unverified original idea, and directed my attention to the singular colouring of the venous renal blood; and when I had noted it well and assured myself that there was no source of error in my observation, I naturally asked myself what could be its cause. As I examined the urine flowing through the urethra and reflected about it, it occurred to me that the red colouring of the venous blood might well be connected with the secreting or active state of the kidney. On this hypothesis, if the renal secretion was stopped, the venous blood should become dark: that is what happened; when the renal secretion was re-established, the venous blood should become crimson again; this I also succeeded in verifying whenever I excited the secretion of urine. I thus secured experimental proof that there is a connection between the secretion of urine and the colouring of blood in the renal vein.

But that is still by no means all. In the normal state venous blood in the kidney is almost constantly crimson, because the urinary organ secretes almost continuously, although alternately for each kidney. Now I wished to know whether the crimson colour is a general fact characteristic of the other glands, and in this way to get a clear-cut counterproof demonstrating that the phenomenon of secretion itself was what led to the alteration in the colour of the venous blood. I reasoned thus: if, said I, secretion, as it seems to do, causes the crimson colour of glandular venous blood, then, in such glandular organs as the salivary glands which secrete intermittently, the venous blood will change colour intermittently and become dark while the gland is at rest, and red during secretion. So I uncovered a dog's submaxillary gland, its ducts, its nerves and its vessels. In its normal state, this gland supplies an intermittent secretion which we can excite or

stop at pleasure. Now while the gland was at rest, and nothing flowed through the salivary duct, I clearly noted that the venous blood was, indeed, dark, while, as soon as secretion appeared, the blood became crimson, to resume its dark colour when the secretion stopped; and it remained dark as long as the intermission lasted, etc.

These last observations later became the starting-point for new ideas which guided me in making investigations as to the chemical cause of the change in colour of glandular blood during secretion. I shall not further describe these experiments. . . . It is enough for me to prove that scientific investigations and experimental ideas may have their birth in almost involuntary chance observations which present themselves either spontaneously or in an experiment made with a different purpose.[1]

We have here the sequence which Bernard outlined: (1) the chance observation, (2) the idea born in his mind, (3) the reasoning which led to the devising of an experiment, (4) the observation of result, (5) the devising of new experiments which eventually led to the far-reaching discovery of the vasomotor system. As L. J. Henderson has said, it is not the least of the merits of the *Introduction* that we have in it "an honest and successful analysis of himself at work by one of the most intelligent of modern scientists."[2]

The expository style of the *Introduction* has enough distinction to earn for it a place by its own right in the history of French literature, but the book has also a curious secondary association with that history. In 1868, three years after its first publication, Émile Zola sketched out the plan for the remarkable series of novels in which he undertook to apply the methods of

[1] viii, 272–5.

[2] *Introduction to the Study of Experimental Medicine,* trans. by H. C. Greene, introduction by L. J. Henderson, p. v.

science to creative writing. The naturalistic or experimental, as distinguished from the merely realistic, novelist was, like the scientist, to be an observer and an experimentalist combined. It is not necessary to develop here the fallacy of Zola's position—that the "experiments" of the novelist can be carried out only by and within himself—but one may be permitted to complain that the essay entitled *The Experimental Novel*, in which Zola undertook to base his theory of fiction on Bernard's *Introduction*, is a fantastic caricature of the brilliant lucidity of the original.[1]

After the publication of the *Introduction* had given Bernard a literary as well as a scientific reputation he began to contribute occasionally to the *Revue des deux mondes*, and these essays, which were assembled posthumously with a few other papers in *Experimental Science*, were chiefly devoted to the restatement of his views on determinism, with, however, an increasing emphasis, as time passed, on the autonomy of physiology as the science of the phenomena of life. *Progress in the physiological sciences* was published in 1865, *The problem of general physiology* in 1867 and *The definition of life* in 1875. His *Report on the Progress and Achievements of General Physiology in France* of 1867 undertook to drive home the point that special facilities must be furnished to the physiologist on account of the special nature of the subject matter of his science, and this led to the inclusion in that pamphlet of an account of the philosophical presuppositions which Bernard considered to be needed to explain his conception of the unique qualities of organized living beings. The ripest expression of his general views is to be found in the

[1] There is a story in connection with Zola's "Nana," which dealt with the *demi-monde* of Paris, that a reviewer questioned him about the amount of personal investigation which had gone into its composition, and he is said to have replied that he had once lunched with an actress of the *Variétés*.

first lecture of those collected in *Phenomena of Life Common to Plants and Animals* of which the proofs were corrected in the last year of his life.

In the first four of these sources Bernard presents his ideas in approximately the same order. He states that life is no obstacle to experimental analysis, that living beings are subject to absolute determinism. They are in that sense machines. Nevertheless, the distinction between the animate and inanimate bodies cannot be ignored. What is Life? Bernard's first answer is the spectacular one which sees life as the other side of death. "Life is creation," he says in one place; and again, "Life is death!"[1] This to Bernard is only an apparent paradox; the ideas are really complementary, not antithetical. He divided phenomena which take place in the living being into the processes of creation, organization and nutrition, on the one hand, and those of disintegration and destruction on the other. Both sets of processes are open to chemical analysis. Nevertheless, turning as he did from the mere analysis of organic substances which had absorbed his immediate predecessors to the investigation of the working organization of the living being as a whole, Bernard found himself not quite satisfied. He pointed out that living organisms have anatomical apparatus and organic tools peculiar to them.[2] His exact words in the essay on *The problem of general physiology* are: "In the living being phenomena are realized by the aid of vital processes and organized chemical reagents created by histological evolution, which, therefore, are special to the organism and cannot be imitated by the chemist."[3] It would seem that he had in mind those peculiar chemical substances of which

[1] xvi, 40–41. [2] xviii, 114; ix, 136, notes 212–14. [3] xviii, 115.

hæmoglobin, adrenalin and thyroxin are familiar examples.

Had he not been so possessed by Descartes' idea that the living organism is a physical machine, and had he had more faith in his own conception that the living organism is maintained by a balance of chemical reactions, he would, perhaps, have realized that the synthesis of the first peculiarly organic substance, urea, which was successfully accomplished by Wohler when Bernard was in his 'teens, would be followed by the synthesis of many of these compounds "peculiar to the organism." Indeed, as we have already seen, he himself was on the way towards isolating the ferment of yeast cells when he was interrupted by death. Nevertheless, because of the state of science in his day and his historical background of general ideas, he felt it necessary to restate the old antithesis between organization and mechanism. He credited the special morphology of living beings to "legislative or evolutive forces," which were in turn subject to primary causes, inaccessible to scientific investigation. On the other hand, his "executive forces" were the same for animate and inanimate phenomena. These were the secondary causes, or conditions, open to scientific experimentation. The character of his distinction between primary and secondary causes is shown in his statement that "In vital and physical phenomena alike a complete knowledge of the *conditions* of their existence, i.e., of their secondary causation, tells us nothing of their ultimate nature."[1] This is, of course, phenomenalism. Although Bernard nowhere uses the label, disapproving materialists who were his contemporaries had not failed to point out that he had taken up his abode in what they considered a temporary shelter for the

[1] xviii, 58.

timid between the old spiritualism and their new creed.[1] M. Littré, too, made it clear that there was no room for this kind of agnosticism in positivism. The positivist had recourse to Comte's system of hierarchies when he wished to state in what way the study of vital phenomena is not *complete* when we have discovered its whole determinism in terms of physics, chemistry and mechanics; and Bernard seems not to have been in any way impressed by this device. M. Littré was also critical of what he regarded as a discrepancy in Bernard's writings between the doctrine which expresses our knowledge of living bodies in terms of a general physics, chemistry and mechanics, and the doctrine which assigns special properties to living bodies. He considered that he differed from Bernard in being ready to explain the first in terms of the second, rather than the second in terms of the first.[2]

It was particularly when Bernard was faced with the problem of the development of the egg that he felt the need of the recognition of the directive forces of life, and he says that in the last analysis the *quid proprium* of life is the *propriété évolutive*, by which he means the capacity of the egg to develop into the adult.[3] It is phraseology of this kind, especially in the *Phenomena of Life Common to Plants and Animals*, which led Driesch to claim Bernard as a critical vitalist.[4] The most uncompromising passage may be translated as follows:

> In spite of the fact that (vital) phenomena are connected with physico-chemical manifestations, the question in its essence is not thereby clarified; for it is not a fortuitous encounter of physico-chemical phenomena which fashions

[1] *Rev. scient.*, 17: 303, 1879. [2] *Rev. de phil. positive*, 21: 5–11, 1878.
[3] xviii, 210. [4] Driesch, H., *The History and Theory of Vitalism*, pp. 132–7.

each living being according to a plan and after a design fixed and foreseen in advance, and gives rise to the admirable subordination and harmonious concert of the acts of life.

There is in the living body an arrangement, a sort of disposition which cannot be slurred over, because it is really the most striking character of living beings. That the idea of this arrangement is poorly expressed by the word *force* we agree: but here the word makes little difference, it is enough that the reality is indisputable.

Vital phenomena have beyond question their rigorously determined physico-chemical conditions; but at the same time they are subordinated to and succeed one another in a succession and according to a law which are preordained: they are eternally repeated with order, regularity and constancy, and they are harmonized with a view to a result which is the organization and growth of the individual, be it plant or animal.

There is as it were a pre-established design of each being and of each organ, so that if, taken by itself, each phenomenon in the organization is tributary to the general forces of nature, taken in its relationship with the others, it reveals a special bond, it seems to be directed by an invisible guide on the path which it follows and to be brought to the place which it occupies.

The simplest reflection shows us a character of the first importance, a *quid proprium* of the living being, in this pre-established vital economy.[1]

The best antidote to the almost rhetorical quality of the language here is, perhaps, a remark at the end of Bernard's essay, *The definition of life*, written three years before his death:

In saying that life is the directing idea and evolutive force of the living being I express merely the idea of a *unity* in the succession of all the morphological and

[1] xvi, 50–51.

> chemical changes accomplished by the germ from the beginning to the end of life.[1]

The notion of organic unity is the valuable aspect of his thought, and this he carried over into the fruitful conception of the *milieu intérieur*.

[1] xviii, 210.

CHAPTER TWENTY

THE INTERNAL ENVIRONMENT

Tous les mécanismes vitaux, quelques variés qu'ils soient, n'ont toujours qu'un but, celui de maintenir l'unité des conditions de la vie dans le milieu intérieur.[1]

BERNARD HIMSELF SPOKE of the conception of the internal environment as the "basis of general physiology,"[2] and he considered that he had been the first physiologist to grasp it. He said:

> That an exterior environment was necessary to the life of the organism has always been recognized. But I have not observed that anyone before myself has distinguished an exterior and an interior environment. I think that I have been one of the first to propose and develop this idea of the blood considered as an interior environment of the organic elements.[3]

This was written in 1867 and he added that he had included an exposition of the "interior organic environment" in his lectures at the Sorbonne for the past twelve years. The earliest reference in the published lectures is in *Physiological properties and pathological alterations in the liquids of the organism*, delivered at the Collège de France in 1857.[4] The conception was fully developed in the *Introduction to the Study of Experimental Medicine*[5] and it is stated in its most general, or, to use the late Dr. J. S. Haldane's word, its most "pregnant" form in the *Phenomena of Life Common to Animals and Plants*: "All the vital

[1] xvi, 121–2. [2] viii, 109. [3] ix, 182.
[4] vi, 43; cf. x, 54; xi, 434; xiii, 7. [5] viii, 107–12.

mechanisms, varied as they are, have only one object, that of preserving constant the conditions of life in the internal environment."[1]

The essence of the idea of the internal environment is that the cells within living bodies, particularly of the higher organisms, are bathed by fluids which constitute an inner environment. Life is possible only if the composition of these fluids varies within extremely narrow limits. The tendency of the living body is to maintain as constant as possible the composition of this internal environment, and if the dynamic equilibrium is slightly upset in one direction, reactions take place which tend to restore the balance. A higher organism is, therefore, virtually independent of its external environment. It is, as it were, "enclosed in a hot-house," so that "the perpetual changes of its cosmic environment do not reach it; it is not chained to them; it is free and independent."[2]

This is Bernard's great biological generalization, a freedom within limits. In his own time more attention was attracted by another generalization which we no longer consider to have quite the significance which he thought it had, namely, that animals as well as plants manufacture sugar; and even in the next scientific generation Sir Michael Foster was content in his biography of Bernard to make the barest passing mention of the "internal medium," as he translates *milieu intérieur*.[3] But in the last decade the conception of the internal environment has captured the physiological imagination. The late Dr. J. S. Haldane was, perhaps, the pioneer in developing what he considered to be its philosophical implications for biology. He thought that both vitalism and mechanism had erred

[1] xvi, 121–2. [2] xvi, 113.
[3] Foster, M., *Claude Bernard*, p. 157.

by separating the living organism and its environment in observation and thought. The discarded vitalism of the early nineteenth century had ignored the ultimate dependence of the living organism on conditions outside it, and the mechanism which succeeded that vitalism neglected the integrative processes mediating the relationship between the organism and the cosmic environment. Haldane said:

> The apparent action and reaction between organism and environment has a distinguishing character which prevents us from regarding it as simply action and reaction. The apparent actions and reactions are, when regarded as a whole, seen to be normally so co-ordinated that what appears as structure in the organism is actively maintained. This structure and activity cannot be separated. The actions cannot be separated from what seem to be innumerable other simultaneous actions and reactions, so co-ordinated as to express the maintenance of the structure. Hence in interpreting the phenomena we cannot apply the physical conceptions of action and corresponding reaction, or reciprocal action between self-existing units of matter and energy. We must regard the phenomena as being, in so far as we understand them at all, the active manifestation of a persistent whole; and the whole is what we call the life of the organism, or the stock to which it belongs.[1]

We are here, perhaps, already beginning to tread on controversial ground, and Haldane's full interpretation admittedly goes beyond Bernard, for he says:

> Bernard regarded the blood (and lymph) as an internal environment bathing all the living cells in the body. In reality, however, the environment of each cell depends on the influence of other cells, so that properly speaking there is no common internal environment, but only a common

[1] Haldane, J. S., *The Philosophical Basis of Biology*, p. 13.

element in environment. Thus the blood bears to actual cell environment a similar relation to what the external environment does, but of a much closer and more defined sort.[1]

The idea of the internal environment has also in recent years had a marked influence on the method of certain very involved physiological investigations. As has been remarked, Bernard's original conception was that the *blood* constituted the *milieu intérieur*, and Professor L. J. Henderson has followed out this idea in an attempt to show the adjustments taking place simultaneously in six of the components of blood which are linked to each other in a twenty-sided equilibrium.[2] He has represented the results in his now famous nomogram. Cannon also, although in a less mathematical manner, has discussed the action of the autonomic nervous system from a similar standpoint, and has shown how his many years of experimentation have all been directed to the demonstration of the validity of Bernard's conception of the internal environment.[3]

Finally, still more recently there has appeared Barcroft's *Features in the Architecture of Physiological Function*, which takes as its text this quotation from Bernard, "*La fixité du milieu intérieur est la condition de la vie libre.*"[4] Henderson and Cannon had been working towards the establishment of the first part of Bernard's statement, i.e., proof that the internal environment is fixed. Barcroft, on the contrary, was struck by the contrasts of the two adjectives "fixed" "free." The answer he finds is most illuminating, particularly for the psychologist. Gross variations in

[1] Haldane, J. S., *The Philosophical Basis of Biology*, p. 73.
[2] Henderson, L. J., *Blood: A Study in General Physiology*.
[3] Cannon, W. B., *The Wisdom of the Body*.
[4] xvi, 113.

the internal environment do not result in devastating disturbances in such body functions as heart action, muscular efficiency, kidney function, etc.; rather, variations in the internal environment result in mental disturbance, lack of ability to concentrate, to think logically, to pay attention. One can therefore expect to find high intellectual development only in an organism whose internal environment has become fixed, so that the intellectual ascendancy of man with his *vie libre* is conditioned by the fixity of his internal environment.

Considered in the light of what he had actually been able to learn about the complex integrative phenomena of life and what has since been accomplished, Bernard's conception of the internal environment was as much a prophecy as a deduction from his own investigations. He had often expressed a disdain for generalization, which suggested that he regarded it as an activity almost unworthy of a scientist, but there is a sort of generalization which can be achieved only by an intelligence rendered clairvoyant by complete familiarity with a chosen subject-matter. Bernard, without any grandiose intention, seems, in proposing his general conception of the internal environment, to have succeeded in providing a groundwork for some of the most productive investigation of a later generation and for an interpretation of the ultimate characteristics of the phenomena of life. It is perhaps desirable that we should remind ourselves that Bernard himself, in envisaging a future for his theory, remained strictly within the limits of a purely practical philosophy:

> If it happens some day that by virtue of patience and hard work, physiology does definitely become established as a science, then we shall be able by modification of the

internal environment, i.e. the blood, to exercise our will on all this world of elementary organisms composing our body; when we know the laws which control their diverse relationships, we shall be able to regulate and modify to our taste vital manifestations.[1]

[1] xviii, 314.

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